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[TRANSACTIONS OF THE AMERICAN INSTITUTE OF MINING ENGINEERS.]

DISCUSSION

OF THE PAPERS OF C. P. SANDBERG ON "RAIL SPECIFICATIONS AND RAIL INSPECTION IN EUROPE," OF C. B. DUDLEY ON THE "WEARING CAPACITY OF STEEL RAILS IN RELATION TO THEIR CHEMICAL COMPOSITION AND PHYSICAL PROPERTIES," AND OF A. L. HOLLEY ON "RAIL PATTERNS," AT THE PHILADELPHIA MEETING, FEBRUARY, 1881.*

ASHBEL WELCH, LAMBERTVILLE, N. J.: Dr. Dudley has given the wear of steel rails under four different conditions. He arrives at the conclusion that the softer rails, or those that from their composition ought to be softer, wear better than the harder. But there is another condition which has an important bearing on the subject, and should not be overlooked,—the weight on a wheel. With the lighter weights of the past, the softer rails may have worn best; with the heavier weights of the future, the harder may wear best. Weights will probably be increased up to the capacity of steel to bear; then doubtless the harder steel will wear best.

A leaden rail with ten pounds on a wheel might carry millions of tons, but with 100 pounds on a wheel, it would be destroyed by a few thousand tons. So in the days of iron rails, my experience was that the softer rails under light machinery stood better than some of the harder; but under heavy machinery the softer were much the most rapidly destroyed. It is doubtless the same with steel.

The pounding motion of the wheels loosens or spreads the particles of a thin film of steel; the pull lengthwise on the rail de-

* The remarks as here given, as in the previous discussion of Dr. Dudley's papers, have all been written out or revised by the participants in the discussion, and represent, therefore, their mature views. It has been thought that this plan, when it can be carried out without doing any of the speakers injustice in debate, is much to be preferred to a strictly verbatim report. The remarks of Mr. Chanute were sent to the secretary after the meeting, and although they did not form a part of the actual discussion, there can be no doubt of the desirability of including them in this report.

T. M. D., *Secretary.*

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taches or scrapes them off. The softer the metal the more liable the particles to spread or flow sideways; the more brittle, the more liable the particles to break loose. With light machinery flowing may be practically nothing, with heavy machinery it may be enough to wear the rail out very rapidly. At a certain point doubling the weight might increase the flow tenfold. The harder the metal without decrease of tenacity or increase of brittleness, the better we should expect it to wear. All may depend on what is left in or used to make it hard.

Dr. Dudley's observations give us incidentally the difference in wear per million of tons carried, caused by difference of weight on a wheel. On the south or loaded track the gross tonnage was 8,000,000 per annum, on the north or light track 5,000,000. As about the same number of wheels must have gone one way as the other, the average weight on a wheel must have been sixty per cent. more on the loaded track than on the light. The wear per million of tons gross load, as found by summing up the wear on each rail, averages 31 per cent. more on the loaded than on the light track. So in this case the wear per ton of gross load increased as the 0.6 power of the weight on a wheel. With iron rails in former years it increased much faster than this rate. As machinery becomes heavier it will doubtless increase faster with steel.

As the same engines and tenders with the same weights on a wheel passed over each track, and as the speeds were probably greatest on the light track, the difference in wear due to difference in weight on the freight-car wheels was probably greater than above estimated. The former weights on a freight-car wheel was about $(\frac{20000+20000}{8}) = 5000$ pounds. The weights now coming into use are about $(\frac{24000+40000}{8}) = 8000$ pounds, an increase of 60 per cent. over the weights when the wear reported took place. This may entirely change the relative rates of wear of hard and soft steel.

Several interesting inferences seem to be deducible from Dr. Dudley's observations, but I confine myself to the single point I have made. Ten years ago I found the wear of steel on the roads between Philadelphia and New York about 50 per cent. more than Dr. Dudley found it on his road. This is accounted for by the better roadbed and smaller proportion of passenger trains at high speed on the Pennsylvania Railroad, by the narrow-gauge cars on wider-gauge roads as it then was, and especially by the steel rails between here and New York being then only in the worst places.

The softer Sheffield rails made from Swedish pig wore better than either of the two harder kinds I got from France.

Dr. Dudley's conclusion seems to be that rails should approach the condition of Bessemer iron. I found that the wear of iron rails from Bethlehem was about 25 per cent. more than that of steel from Sheffield, laid in the same track and under the same circumstances. This, however, does not show the relative durations of the rails, for steel owing to its elasticity is only injured on the surface, and will wear till it is reduced to a skeleton; while iron is affected all through by each blow, and will finally go to pieces before it is worn down. The point I make is, that though the softer steel may have worn best under the lighter machinery of the past, it does not follow that it will wear best under the heavier machinery of the future.

The patterns of steel rails now displayed, and the papers of Mr. Sandberg and of Mr. Holley, with the allusion that has been made to the history of the now accepted forms, make it proper to give a short historical notice of the first rails which had certain characteristics common to all these patterns, and of the principles on which they were made.

The early steel rails, copied after the iron, had very heavy bases and stems, and no flat surfaces for fishing. One fifth to one eighth of the metal put into them did little or no good, and the fish splice, then coming to be recognized as the best, could not be used to advantage. The useless weight made steel rails so expensive that they came into use very slowly. In 1865 I made a pattern to avoid these faults, guided by the following considerations: The theory then was, and I suppose is yet, that a blow on iron such as that given by a locomotive wheel, is felt all through the metal, and produces a permanent though minute disintegration or change of form, the accumulations of which must in time weaken and *unweld* the base and stem as well as the head of the rail, and so extra metal must be put into the lower part of the rail to compensate for this gradual weakening; but that in steel, owing to its elasticity, such a blow produces no permanent effect on the metal except at the surface. Therefore the stem and base of steel may be very much lighter than of iron, not only on account of its greater strength and freedom from welds, but also from its immunity from deterioration by use. Hence all the metal possible should be in the head where the wear is; and as little as consistent with safety in the stem and base where there is no wear, and with steel, no deterioration. In England where the supports are far apart strength and stiffness are

primary considerations; in this country where the supports are close together, other considerations engross attention. The width of the base should be determined by the endurance of the wood it is to set on, and for this purpose must be much greater than is necessary for strength or to prevent upsetting. Partly from calculation and partly from observing the behavior of iron and steel in circumstances somewhat similar, I made up my mind that three-eighths of an inch thickness of stem was ample to bear the weights and the shocks, vertical and lateral, of the machinery then in use; and that an eighth thickness at the edge of the base was sufficient to transmit to the wood all the pressure its fibres would bear.

In accordance with these views, but conceding something for the sake of abundant safety and the facility of manufacture, I made a pattern with 4 inches height, 4 inches base, head $2\frac{3}{8}$ wide $\times$ $1\frac{1}{4}$ deep, stem 7-16 thick, and edge of base 3-16 thick; weighing 53 pounds to the yard. Assuming that fishing made the best joint, I made (as I had previously done in very slender iron rails) the under side of the head and top of the base plane surfaces, as broad as possible, so as to give a perfect and broad bearing to the edges of the fish-plates, and as near horizontal as possible, so as to lessen the tendency of the fish-plate to work out by the jar.

This pattern was condemned by every engineer to whom it was shown, on the ground that it was too weak and could not be rolled. I was fully aware of the difficulties of manufacture, but was confident they could be overcome. In one point I yielded too much to the manufacturers; rounding off the corners on the under side of the head.

After long negotiations an order for 200 tons for trial was accepted by Naylor & Co., in August, 1866, and sent to England to be executed. There everybody that saw it condemned it, and for several months John Brown & Co. refused to roll a rail of such a preposterous shape. As I wished to have an extreme test of the correctness of my ideas, I insisted on the performance of the contract. At last the rails were rolled, tested by Kirkaldy in London, and in the spring of 1867 laid down in some of the hardest places between this city and New York. Several hundred tons of the same pattern were laid the next season. I continued to watch and test them carefully for eight years, and so far as I could find not one ever broke, bent, or compressed in the stem, or gave out in any other way.

Thus the principles on which this pattern was made, and its proportions with the machinery then in use, were shown to be correct

as seen from the consumers' point of view. It had as big a head and wore as long, and was as free from accident as previous steel rails twenty per cent. heavier. Some manufacturers in this country condemned the pattern because, without trial, they believed it could not be safely rolled. But John Brown & Co., after trial, believed it could be, and showed their confidence by soliciting an order for ten thousand tons more of the same pattern.

The principles of this pattern, with the proportions slightly modified, were gradually, and are now generally adopted. Mr. Hinckley after a year's observation, adopted the pattern with a very slight addition to the metal below the head, and in 1868 relaid one track with it from this city to Baltimore, where the rails may still be seen.

In 1874 Mr. Chanute on the Erie, and Mr. Sayre on the Lehigh Valley, simultaneously and without concert, adopted the sloping sides of the head, which important feature is now in general use.

In 1870 Sandberg's patterns were published, embodying the same principles with the angles and proportions slightly different. Whether he knew what had been done before I do not know. Doubtless the same considerations on which I acted occurred to many others.

As the machinery is now much heavier than in 1866, and steel not now so good as that made by John Brown & Co., from Swedish pig, and as the price of steel is very much lower, a small saving in weight is of less importance. I have nothing to say against the somewhat heavier proportions now generally used—for example $\frac{1}{2}$ -inch stem instead of $\frac{7}{16}$ and $\frac{1}{4}$ -inch thickness at edge of base instead of $\frac{3}{16}$.

In Sandberg's new patterns of 1878, he has however adopted almost the identical proportions I used in 1866. His thickness of stem is exactly, and thickness of the edge of the base is very nearly the same. His fishing angle, which in 1870 was 22° is now 30° —mine was always 28° . It is gratifying to find that so able an engineer, after so wide and so long experience, has within the last three years settled down upon the proportions I adopted fifteen years ago, and which before they were tested were so universally condemned, especially in the very dimensions now adopted by him.

I think Sandberg's heads are too convex, and his bases rather narrow. In America bases are always far wider than stiffness and stability require; the practical question being what width of bearing does the timber require. Where chestnut ties are used the base should be not less than four and a half inches. There is therefore little practical relation between the height and base.

The old plan was to increase every part of a rail much in the same proportion. But each part should be in proportion to what it has to do. The head should be deep in proportion to the amount of traffic and the lowness of the rate of interest on its cost. The body need only be strong enough to carry the head after it is well worn down, and that depends on the weight of the machinery, and in the case of steel has little to do with the volume of traffic, except so far as that affects weight of machinery. As on most railroad systems, the same machinery is used on main lines with heavy traffic, and branches with light, I suggested in 1874 that each system adopt the same body of rail for both, and make the head deeper on the main lines, shallower on the branches. This plan was adopted on the Pennsylvania Railroad. Sandberg seems to recognize this in his patterns of 1878.

R. W. HUNT, TROY, N. Y. : Again Dr. Dudley presents to our consideration a series of most carefully conducted experiments, and while I fully appreciate the labor and thought this work has cost him, I must still hesitate to accept his deductions.

This paper differs from the previous one in that it deals exclusively with the wearing qualities of steel rails. Dr. Dudley gives his reason for this change in the following statement: "With the improvement in maintenance of way which has characterized the Pennsylvania Railroad during the last five or six years, the removal of rails from the track from the first two of these causes (*i. e.*, broken and crushed), has, if I am right, quite notably diminished. This certainly is true with regard to broken rails. And if, as time advances, the number of crushed rails shall diminish, both because of the continued improvement in maintenance of way before referred to, and because, owing to improved and better methods at the steel works, there are fewer crushed rails caused by physical defects in the steel, the question of the wearing capacity of steel rails obviously becomes the all important one."

Certainly the condition of the roadbed has much to do with rails breaking, crushing, and wearing. If the Pennsylvania and other railroads have done and are doing their part, Dr. Dudley courteously admits that the steel works have performed part of theirs. But as far as I am informed, the formulas which they have used have been and are still quite wide of the one he continues to recommend. The rails which are not breaking or crushing to so great an extent as formerly, contain higher percentages of both carbon and manganese.

I will venture the assertion that Dr. Dudley's road has put in but few rails during the last eighteen months that have not contained fully 0.35 per cent. of carbon and 1 per cent. of manganese. And probably the use of this formula will considerably ante-date the time mentioned. These rails do not break or crush, because they were laid upon a better roadbed, were rolled from sounder ingots, were carefully hot-straightened, and, if I may be permitted a Hibernianism, were cold-straightened while still hot. But how will they wear? For an answer to that we must wait.

I have endeavored to study carefully Dr. Dudley's paper, but have failed to be convinced of the correctness of the conclusions which he draws from the chemical analyses and physical tests. I think I have no prejudice in this matter. The best formula for steel for rails is as earnestly desired by me as by any consumer of such steel. In my judgment averages in such investigations are exceedingly dangerous, unless made from an immense number of samples taken from metal that has had exactly the same history. By this I mean, the different samples ought to have been blown at about the same temperature, cast under the same conditions, heated alike, rolled at the same heat, and under the same reductions, hot and cold finished alike, placed upon the same roadbed, and given the same amount and kind of wear. This is almost an impracticable proposition, but the failure to fulfil it, can only be compensated by an immense number of other samples.

Some of us told Dr. Dudley before that his twenty-five samples were too few upon which to build up a theory, and it seems a little ungenerous to make the same charge against his present sixty-four; but I must do it. These sixty-four tests are not taken from rails which have been subjected to the same conditions; on the contrary, "sixteen of these rails were taken from level tangents and sixteen from level curves, eight from the high side and eight from the low side of the curves. Again, sixteen rails were taken from grade tangents and sixteen from grade curves, eight from the high side and eight from the low side of these curves." Asking for so great a number of tests means a tremendous amount of work and patience, but Dr. Dudley has taken up one of the most difficult problems, and must not be contented with a partial investigation. It took some of us a long time to learn how to make Bessemer steel at all; he must not expect to be able to so easily teach us how to make the best.

To illustrate why I object to his averages: I find among the

rails taken from a level tangent that the one which shows the least loss in section and the least wear per million tons of traffic had carbon, 0.423; phosphorus, 0.127; silicon, 0.083; manganese, 0.708; while another rail presenting within three of the worst results, had carbon, 0.428; phosphorus, 0.109; silicon, 0.038; manganese, 0.870. The next poorest had carbon, 0.452; phosphorus, 0.144; silicon, 0.037; and manganese, 0.708,—the manganese being exactly like the best. The poor steel had also very slightly the greatest density. The difference between 0.452 of carbon, and 0.423 and 0.428 could easily be caused by rolling one steel at a higher heat than the other. Again, on the low side of level curves I find the second best rail had carbon, 0.454; phosphorus, 0.145; silicon, 0.015; manganese, 0.726; while the poorest rail had carbon, 0.497; phosphorus, 0.136; silicon, 0.062; manganese, 0.724. I cannot now believe that the slight difference in the chemical constituents of these rails caused the great difference in their wear. If such is the case, then I for one stand appalled at the difficulties which surround the making of a perfect rail.

As before stated averages are dangerous. Dr. Dudley makes up a formula from the averages of his investigations, but admits that the silicon percentage is disturbed by an abnormal piece of steel No. 881. This had carbon, 0.483; phosphorus, 0.035; silicon, 0.480; manganese, 0.782; and stands eighth in sixteen tests. It may be remembered that in the discussion at the Baltimore meeting in February, 1879, I mentioned a rail then in the track of the Boston and Albany Railroad that contained carbon, 0.360; phosphorus, 0.124; silicon, 0.469; manganese, 0.571; and which had then been in the track five years. That rail is still in service and in good condition. Here we have two rails made by widely separated works, laid in the tracks of railroads hundreds of miles apart; one giving eleven years and one month of service and then not worn out, but on the contrary selected as a "slow-wearing rail;" the other one good after over seven years of wear. Will we take the average composition of these rails which did not break, nor crush, nor rapidly wear out, and assume that carbon, 0.420; phosphorus, 0.079; silicon, 0.474; and manganese, 0.676 is the proper formula for rail steel? If not, why not?

Dr. Dudley's averages of his 32 samples of good wearing rails gave him carbon, 0.334; phosphorus, 0.077; silicon, 0.060; manganese, 0.491. If that formula is right, stick to it. In fact such a one will be better for both the producer and consumer, than

the compromise which Dr. Dudley recommends. I think every steel-maker will bear me out in saying that sound steel can be more easily made under its provisions than with carbon from 0.25 to 0.35; phosphorus, 0.10; silicon, 0.04; manganese ranging from 0.30 to 0.40 aiming at 0.35. Theory is very fascinating, but in practice stubborn facts present themselves, and with steel containing 0.10 phosphorus, and not more than 0.35 manganese, the resulting ingots would be very unsound, and the rail mill would produce an indefinite number of imperfect rails, many of which would get into service in defiance of the most careful inspection; the result being crushed ends, flat places, and generally unsatisfactory rails. If low manganese is desired, the phosphorus must also be low; 0.10 per cent. cannot be so considered. In the 64 analyses there are but 16 with the manganese as low as 0.40 and under; and only 4 of these have the phosphorus above 0.085; 11 being under 0.07 and 6 under 0.05. The rails having less than 0.30 carbon, with the exception of 6, were made over 12 years ago, and of these 6, one was made 8 years, one 10 years, and four about 11 years ago.

At the time all of these rails, excepting one, were made, all steel was hammered, the blooming mill not having been invented. Under the hammer it is possible to coax steel into fair-appearing blooms, that would either go to pieces or roll very badly in the blooming mill. When the latter was introduced the steel makers had only at their command recarburizers poor in manganese and high in phosphorus. Moreover the American irons were then even much higher in phosphorus than our chemists told us. Hence a great deal of very poor steel was made. By poor I mean unsound steel. As high as 20 per cent. second quality or defective rails was a common run of work. While to-day with better irons, richer spieghels, and better melting furnaces, we rarely exceed 1 per cent., and run for days at less than one-half of that figure and this on much more difficult sections than railroad engineers formerly required.

Just here let me ask Dr. Dudley whether he gave sufficient consideration to this very difference of section? Examples are presented by rails 902 and 903. The first stands at the head of rails from the high side of curve grade, and gave upon analysis: carbon, 0.322; phosphorus, 0.077; silicon, 0.026; manganese, 0.492. The second had carbon, 0.355; phosphorus, 0.108; silicon, 0.029; manganese, 0.490, and is at the foot of the list with but 2 years 11 months service, while the first had 7 years 11 months, and was of

the old rounded head section, while the poor one, with almost the same chemical analysis, was of the square head pattern.

If, as suggested by Dr. Dudley, even softer rails are desirable, there need not be any difficulty in filling such an order. I would be perfectly willing to contract to make rails containing not over, carbon, 0.15; phosphorus, 0.08; and manganese, 0.50. These rails would be perfectly homogeneous and stiffer than an iron rail of the same section, and ought therefore to hold up the load. The rail makers both in this country and England are now making steel containing much more manganese than formerly. These rails are going out of the mills, apparently better and sounder than those formerly made. Time only will demonstrate whether or not they are actually better.

The present Troy practice is to use irons containing the least phosphorus, to put in enough carbon to make strong steel, and enough manganese to make the steel roll sound, both while in the ingot and the bloom, to carefully heat the ingots and resulting blooms, hot straighten the rails so as to leave the minimum of work for the cold press, which does its work while the steel is yet hot. And we have to be yet convinced by the wear of our rails that this practice is wrong. For our tests we cast a four-inch ingot from each blow; this is hammered into a $\frac{5}{8}$ -inch bar, which when cold is required to bend to at least a **U** by the blows of a sledge, this bending being a much severer test than when done in a press. The steel is also quenched in water and tested for temper. Drillings are taken from the ingot and *accurate* carbon determinations made.

I prefer this plan to any test of the rail ends. To be perfectly conclusive, such tests would have to be made of both ends of the rail, and from every rail; for one end might be overheated, and the other not. Some blooms might be all right, and the rest of the heat spoilt. In a mill producing say 9000 rails per week, 18,000 rail-end tests would be no inconsiderable item, particularly if, as Dr. Dudley proposes, the test-piece, "12 inches long, $1\frac{1}{2}$ inch wide, $\frac{1}{2}$ inch thick," is to be slotted from the web of the rail. He would have to give a lease of the Altoona shops along with the rail contract.

Dr. Dudley's theory of infinitesimal teeth is interesting, and, if true, I should prefer having the teeth of my rack so strong that they would neither break off or flatten down.

As a matter of perhaps some interest, I present two pieces of Troy rails, cut off at the saws from two rails, not in the same heat, and

tested without knowing anything of their chemical composition. I had these pieces separately placed upon 10-inch bearings under a 7-gross ton hammer, a piece of $2\frac{1}{2}$ -inch round iron laid upon them as a fuller, and the hammer allowed to fall from 20 inches above the fuller, which, according to Haswell, gave a blow of 67.75 gross tons. The pieces were then turned over, the fuller placed upon the convex surface, and the hammer allowed to fall from 13 inches above the fuller, giving a blow of 58.45 gross tons. You will see that the rails do not show any signs of rupture, and their color at the points of torture prove them to have been absolutely cold when the test was made. I think these rails ought to be reasonably safe in the track. As you see by this piece of the head of one of these rails, I had it planed, and then some teeth cut in it by a cold chisel, and one-half of them pounded down with a hammer. The teeth of my rack did not break off. The analyses of these rails subsequently made are :

	I.	II.
Carbon, ...	0.410	0.380
Silicon, ...	0.050	0.058
Phosphorus, ...	0.086	0.082
Manganese, ...	0.942	0.840

In conclusion, I will say that Dr. Dudley's paper as a contribution to our knowledge of steel rails is valuable and interesting, but I protest against his conclusions being received as manufacturing or commercial axioms. From its being presented in the form of a report to the leading railroad of the country from one of its trusted officers, as well as from the tone of the concluding deductions, it is liable to be so received by railroad men. If any railroad company desires rails made under either of Dr. Dudley's formulas, and are willing to pay a price large enough to cover the loss in making them, well and good; but so long as the railmakers are compelled to *guarantee* the wear of their rails for a given number of years, justice requires that the composition of the steel from which these rails are made should be left to them.

WILLIAM SELLERS, PHILADELPHIA: The very interesting paper upon the wear of steel rails that has just been read presents the record of a series of investigations that are extremely valuable, and the deduction that has been drawn from the results noted seems to be unavoidable; the tests, however, to which it is proposed steel rails shall be subjected hereafter, with a view to determine their quality, should

have our careful attention, and with reference to these I desire to make a few suggestions.

While the manufacture of soft steel was yet in its infancy it was believed that the presence of phosphorus in any notable quantity was very deleterious, in fact fatal to the good quality of this product, and the greatest care was exercised to procure materials in which this element could not be more than traced. With the development of this art it has been found that larger and larger proportions of this hurtful ingredient may be used, providing always corresponding changes in the chemical composition shall be made to accord therewith.

It is perhaps beside the point to inquire whether the earlier belief was correct or not, the fact remains that steel is now produced which contains much larger proportions of phosphorus than would have been permitted a very few years ago, and that this steel is now considered to possess qualities which fit it admirably for use in rails ; moreover it is well known that the degree of heat and the manipulation to which the ingot is subjected in transforming it into the finished product has an important influence in determining the characteristics that product will exhibit.

These facts have an important bearing upon the question, what shall be the tests which are to determine the quality we desire to attain. If the engineer is to specify the chemical composition and the mode or process of manufacture by which the manufacturer must work, it would seem that improvement in the art must to a certain extent be limited, and a vicious system would be introduced, injurious alike to the engineer and the manufacturer. The chemical composition has no value to the engineer, for no matter what the chemical composition, it is upon the physical qualities at last that he must rely to determine whether or not it will answer his purpose ; it should be his business therefore to devise such physical tests as will determine absolutely whether or not the quality that he desires has been produced, while the manufacturer should be left free to make such chemical combinations or adopt such processes of manufacture as will fulfil the requirements of the engineer. It may, however, be questioned whether the physical data we now possess will enable us to agree upon the physical tests requisite to demonstrate the quality, but if this is admitted it only proves that further physical data are wanting, for the quality is finally determined by use, the result of which our physical data should enable us to predict. Of these data the one most abundant is

the ultimate strength, and next to this ductility, bending, shearing, punching, torsion, impact, and fatigue, in all of which, except the last, abundant facts are at hand. As most specifications prescribe a high and a low limit for ultimate strength, it would seem to be the prevalent opinion among engineers that high or low steel, as it is technically termed, is to be determined by its ultimate strength, and it becomes important at this stage to define what quality it is that most accurately defines this term, that is to say, does it consist in high or low ultimate strength, or in high or low ductility relatively to its ultimate strength. It is essential for all structural uses that the engineer should know upon what ultimate strength he can rely, for upon this all his calculations must be based, but it is upon ductility that he must depend for safety, the measure of which he must determine; after which, the higher the ultimate strength he can obtain the better his material will be for any structural purpose; that is to say, with a given ductility the higher the ultimate strength of his material, the safer it is and conversely, with a given ultimate strength, the higher the ductility the safer it is. High ductility and high ultimate strength cannot be produced except with the most favorable conditions, both as to chemical composition and as to the mode of manufacture; the quality is therefore to be ascertained with most certainty by determining the relation of the one to the other, and for the same reason this relation would seem to be the factor which should most accurately define the term high or low, as applied to steel; and the requirements simply of an ultimate strength not less than ——— pounds per square inch, with a ductility not less than ——— per cent. would at once determine the character of steel required and the quality of it. It must not be understood, however, that the determination of this relation is the sole requisite in determining quality for every purpose, for the capacity to bear fatigue and shock is scarcely less important, as for example, the question now under consideration; and although it is probable that the material which exhibits high ultimate strength coupled with high ductility will prove to be capable of enduring the most fatigue and shock, we cannot affirm that there is any definite relation between the two, in fact we have many data tending to show that such a relation does not exist.

An examination of the data which Dr. Dudley has tabulated indicates that to establish the relation between ultimate strength and ductility alone would be insufficient to determine the wearing quality of rails, so that these data must be supplemented by some other; this

other, I suggest, should be that of fatigue from shock, not that of simply bending, which last Dr. Dudley "has found to bear a closer relation to the loss of metal per million tons than any of the other tests." I take exception to the classification of this test as one of the four ways in which a bending test could be applied. A rail bent under the drop test, and one bent in the testing machine by pressure slowly applied, would not be subjected to the same character of strains. While a drop test is a bending test it is also much more; the same number of degrees of deflection in the one case as in the other would, I think, represent very different powers of resistance in the material operated upon. With the same weight falling from the same height in properly constructed guides, the same *effect* must be produced with every blow. In fact it is difficult to conceive how any other form of test can produce more uniform effects or which can be more accurately measured; the *results* may be more diverse with such a test than with others because they are produced by pressure and shock, whereas nearly all other forms of testing produce their results by pressure alone. It is this difference, however, which commends the drop as the test above all others for rails as being more analogous to that by which the rail is tested in use, and we should be well satisfied that the objections urged against it are well founded before we abandon it for others which may appear to offer more uniform results. It may well be that uniform results obtained by a system of testing widely different from that we require our material to sustain in use, may have small value for determining in advance the effect of that use. I am thus driven to the conclusion that to obtain the relation of ductility to ultimate strength, together with the capacity to sustain fatigue from shock would be to attain to absolute certainty as to the quality in an engineering sense.

There are, however, considerations other than that of the tests which must have attention before adopting a system of testing for steel rails, and as to these I would now make a few suggestions. The tests required to determine quality in the directions indicated are well understood, but simple as they are, the time that must be consumed in making them, would result in serious loss to the manufacturer if his mill is to be held for their determination, and if he proceeds with the execution of his order in advance he incurs a serious responsibility in assuming the risk of rejection for the large product that would be turned out before the requisite tests could be made. While the cost of a test for ultimate strength, ductility, and for capacity to bear fatigue would be small, the large number of such

tests that would be necessary to establish the quality of an ordinary order for steel rails would be a serious item, and as every item of cost must be eventually borne by the consumer it is important for the railway companies to adopt a regular system for testing their rails that shall not only be the most expeditious but the least expensive. For the purpose of inspection, therefore, it would seem to be sufficient to adopt a system that would be simply an indication as to the qualities desired, without subjecting the parties interested to the cost and delay which must result from exhaustive and thorough tests, and upon these indications the rails might be accepted. This would seem to accord with the best foreign practice, as illustrated by the very admirable paper upon "Rail Specifications and Rail Inspection in Europe," by C. P. Sandberg, C.E., read at the Lake Superior meeting, August, 1880. There are two tests which would give these indications with great accuracy, both of which could be applied without the expense and delay incident to preparation of specimens, and both of which require comparatively inexpensive machinery. These are the registering punch and the drop test. The former is a special tool which could be applied upon the crop ends and could be portable; the latter is too well known to require description, but its indications, if carefully noted would, I believe, be the most valuable of the two, but in conjunction with the punch they should be conclusive. The punching test would have this advantage, that by its use an inexpensive indication could be had of the quality of every rail. The suggestion that this test should be applied upon the fish-plate holes has probably prevented its introduction heretofore, first, because such holes are not now punched, and second, because to make such registration every manufacturer would have to procure a registering punch, and this would be difficult to apply upon existing machines. If this tool should be recognized as a part of the inspector's outfit and specially adapted to his needs, it might soon come into general use if care was used to maintain the punches and dies in good condition.

In conclusion, I suggest that if the physical tests are to be supplemented by chemical analysis the specification for this analysis should not be complete; that is to say, in place of giving the proportions of carbon, phosphorus, silicon, and manganese, a maximum limit should be fixed respectively for phosphorus, silicon, and manganese only, leaving the carbon to be varied by the manufacturer, so that he may properly be required to furnish material that will fulfil the physical conditions, for it is evident that if the engineer defines the chemical

composition, he cannot reasonably ask the manufacturer to guarantee that this composition shall give certain physical results.

W. R. JONES, PITTSBURGH, PA.: The question that naturally occurs to me is this: Has Dr. Dudley in his investigations been aiming to prove a theory, or has he been guided by an earnest desire to discover what are the proper elements in the composition of a good-wearing steel rail?

Unfortunately, for correct chemical information, he has omitted in his analyses two very important elements,—sulphur and copper. Now, before we will even begin to admit the correctness of Dr. Dudley's conclusions and the formula he prescribes, we will at the start question the propriety of any chemist or scientist prescribing a formula for making steel when he has ignored such important elements as sulphur and copper. I, for one, will not accept any such formula.

Are we sure, or is Dr. Dudley sure, that the chemical analyses embodied in his paper are correct? This may seem a presumptuous question, yet, with my experience with chemists, I naturally doubt the correctness of the analyses, and, before I will accept them as correct, I will ask that comparative tests of phosphorus and manganese be made by the Pennsylvania Railroad chemists and the chemists of the leading steel-works in the country. Let us first verify the correctness of the analyses before we consider the conclusions. I can enumerate a great number of instances in which chemists have differed very widely in their determinations of phosphorus and manganese. A prominent iron firm made a contract with the Edgar Thomson Steel Company to deliver pig metal guaranteed to be between 0.07 and 0.08 phosphorus; an analysis by our chemist resulted in phosphorus 0.148 and 0.152,—a rather startling difference! Again, a sample bar of steel, in which our chemist reported phosphorus 0.11, was tested by a chemist of another Bessemer works, and his determinations were phosphorus between 0.07 and 0.08. A leading engineering establishment of Pittsburgh bought iron claimed by a chemical analysis to contain 0.08 phosphorus; our Mr. Ford found phosphorus 0.145. A chemist connected with an open-hearth works reported manganese in a piece of steel, 1.14; in a second determination from the same piece of steel, by the same chemist, manganese was reported 0.43. The chemist was kept in ignorance of the fact that both samples were from the same piece of steel. Two determinations for manganese

were made by the same chemist from a piece of steel; he reported manganese 0.61 and 0.58. Our chemist, in the same steel, reported manganese 0.324 and 0.303. I could give innumerable instances of the wide difference in chemists' determinations of phosphorus and manganese. I think I have cited sufficient cases to sustain the position I have assumed, viz., that before the determinations made by Dr. Dudley's assistants are accepted as being correct they should be verified by chemists of greater experience.

I find on a close examination of the Doctor's paper that he has taken no notice whatever of the increased weight of cars, increased weight of locomotives, with increased speed, in his tonnage calculations. Now, there is a vast difference between the tonnage of 10 to 12 tons in an ordinary freight car with eight wheels passing over rails at a moderate speed, and 15 to 20 tons on the same number of wheels at an increased speed. Since 1874 the Pennsylvania Railroad Company has been steadily increasing the weight of both engines and cars. The duty to which rails are now subjected I believe is fully 60 per cent. greater than that before the year 1874. On looking over the paper, we find a number of rails, classed as good-wearing rails, that have for years been subjected to comparatively light tonnage on a wheel-tonnage basis, compared with a great number of rails classed as fast-wearing rails, which in some cases I find have had passing over them nearly twice the number of tons per month, and all on heavier wheel tonnage.

As an illustration I cite rail No. 937, with the following analysis: carbon, 0.454; silicon, 0.015; phosphorus, 0.145; manganese, 0.726; with only 2 per cent. elongation. In accordance with the deductions and formula this should be a very bad rail, yet on close examination I find that this rail has been subjected to a monthly tonnage of 747,628 tons. If we examine the rail No. 929, which was laid within two miles of rail 937, we find: carbon, 0.235; silicon, 0.080; phosphorus, 0.055; manganese, 0.300; elongation, 24 per cent. This rail was subjected to a monthly tonnage of only 381,235 tons, while rail No. 937 was subjected to 96 per cent. more monthly tonnage, and yet rail No. 929 is classed as a good rail. If we assume that 50 per cent. more loaded cars pass east to Philadelphia than pass west from Philadelphia, we find the wheel tonnage assumes a very important aspect in determining the wearing qualities of rails. Again, the bad rail was in the track forty months, and only shows a wear of $\frac{5}{100}$ of an inch in vertical section, and has

been in the track since the advent of heavier engines and heavier cars; while rail No. 929 has had the advantage of at least seven years of comparatively light traffic on light-wheel tonnage. The question which of these two rails has been subjected to the greatest amount of wheel tonnage I leave to some one to calculate who has more time to devote to this subject than I have.

In regard to the method proposed by Dr. Dudley to test the rails at the works, I can only say I much prefer the methods suggested by Mr. Sandberg, in his paper read before the Institute at the Lake Superior meeting, with this slight modification, viz.: I would subject a 50 and 52 pound rail to a drop-test of 1800 pounds, falling a distance of 14 feet on the rail, on supports 3 feet apart; for a 54 to 56 pound rail, 16 feet drop; for a 56 to 58 pound rail, 18 feet drop; for a 58 to 60 pound rail, 20 feet drop; and so on in the same ratio. I would also adhere to the test-bar, drawn out from the head of the rail down to 1 inch square, then placed under a steam-hammer and bent through an angle of 110° , the distance between centres of supports of the bar to be from 10 to 12 inches. Dr. Dudley may think these tests crude; I believe them to be simple, thorough, effective, and reliable, and in this I fully concur in the views of Mr. Sandberg.

If Dr. Dudley and the Pennsylvania Railroad authorities believe their deductions are correct, let them have rails made in accordance with the Doctor's first formula,—phosphorus, 0.077, carbon, 0.334, silicon, 0.060, manganese, 0.491,—and add to it, the less sulphur and copper the better, and, as a matter of course, pay the difference in price involved in the difference in the price of metals; but when Dr. Dudley attempts to formulate a rule to govern the steelmakers, based on his knowledge, I, for one, decidedly object; and I frankly tell him that he is opposing all the researches and investigations of the best chemists and metallurgists, both here and abroad.

I have serious and grave doubts if steel made in accordance with his second formula would give a good record in the track. I experimented on this formula in attempting to fill an order. Mr. Sandberg in his paper refers to the filling of an order of 2500 tons on the same formula, and my experience was the same as his. The ingot was a conglomerate mass of honeycombs. It made bad blooms, and I do not believe it made good rails. The rails are now in the tracks of the West Pennsylvania road, and if they do prove to be good rails, I shall be very much surprised.

WILLIAM METCALF, PITTSBURG, PA.: In rising to discuss Dr. Dudley's paper, I feel somewhat as I did at the Baltimore meeting—that a "crucible" man has no right to interfere in a "Bessemer" discussion; yet having read the paper very carefully, I feel impelled to say something, for two reasons: First, because I believe Dr. Dudley is entirely on the right track, and having undertaken and partly accomplished a great work, he is entitled to the help of all who have experience in these matters; and second, because the data given force me to concur in Captain Jones's opinion that the analyses are incomplete, since they ignore sulphur, copper, nitrogen, and possibly other injurious elements.

In an experience of fourteen years, and with probably more than a hundred tests, we have never found the chemistry and the physics of crucible steel to disagree. If in any case a disagreement has appeared, it has been our invariable custom to go all over our physical tests with great care, and if we found no error, then to refer the matter back to the chemist who has invariably found some unexpected element to account for the trouble.

It is only just to the chemist to say here, that ordinarily he is only expected to determine phosphorus, silicon, sulphur. Generally the metals, with the exception of manganese, are not looked for, although a watch is usually kept for copper and arsenic. Further, in most cases we have found our own work more liable to be faulty than the chemist's.

Having arrived then at such a degree of experience that we can predict the analysis from our tests, or our tests from the analysis, with almost absolute certainty, I can see no reason why the same results may not be attained in the Bessemer practice. But two things are essential, neither of which we have here; first, complete analyses; and second, a record of the nature of the blow, the heat at which the ingots were bloomed, and the rails finished,—in short, a complete history of the manufacture.

This latter is quite as essential as accurate and complete analyses. Dr. Dudley ignores sulphur and copper on fair enough grounds commercially speaking, but when he announces so grave a conclusion as he has reached, in a scientific way, the omission of any elements that may affect the conclusion is hardly justifiable. His differences of phosphorus units, which I must term units of rottenness as far as phosphorus is concerned, and which I am sorry he did not name "*Units of Alloys*,"—are very small, and if sulphur and copper had been included they might have upset his conclusions altogether. The

omission of nitrogen is not to be criticised in the same way, because it has not been usual to regard nitrogen in testing steels, yet I am forced to believe that nitrogen plays a very important part in Bessemer steel.

We had occasion recently to test some of the finest Bessemer steel that is made, in order to ascertain how far the Bessemer people were encroaching on the domain of the crucible steel manufacturers. In the accompanying table are the analyses of the Bessemer steel referred to, two samples of the very best foreign crucible steel, and one of the best American crucible steel.

Kinds of steel.	Carbon.	Phosphorus.	Silicon.	Sulphur.	Manganese.	Copper.
Bessemer.....	0.400	0.027	0.003	trace.		
Foreign crucible.....	1.295	0.020	0.009	trace.	0.050	
American crucible.....	0.862	0.005	0.007	none.		0.0013
	0.960	0.021	0.033	trace.	0.077	

All three of the crucible steels were of exceptionally good quality. It will be observed that according to the analysis, the Bessemer steel should have been equally good. Upon a careful test of an 0.80 carbon billet it proved to be thoroughly worthless.

The case was then referred to Professor Langley, and he attributed the trouble to oxygen, and predicted that if we would melt a 0.40 carbon billet with a little ferromanganese to remove the oxygen and bring up the carbon to 0.80, and also give a little more silicon which the steel would take from the crucible, that we would have a steel equal to the others given above. To be sure of our work, we melted the billet, the analysis of which is given above, and produced an ingot of about 0.80 carbon, as near as the eye could determine, and that is within .03 in such high steels. The remelted Bessemer steel was just as bad as the other, and of this we assured ourselves by the most careful and repeated tests.

Now we know oxygen was not the cause of the fault, for if it had been, the ferromanganese would have removed it. In the Bessemer manufacture immense volumes of nitrogen are blown through the molten mass, and by the evidence of all of the most eminent chemists who have examined the subject, we know that nitrogen does unite with iron, that the compound is brilliantly lustrous, hard, brittle, and even friable, and that it will harden like steel. We also know that there is a peculiar lustre in all Bessemer steel, which makes it easily distinguishable by an expert from crucible or

open-hearth steel, provided none of them have been overheated. Is it not more than probable then, that nitrogen is entitled to far more serious attention than it has yet received?

Dr. Dudley classes carbon with phosphorus, silicon, and manganese in making up his units, and he may be correct in his make-up in his relative values of each, but I cannot see how he has proved his formula, nor how it is to be disproved with our present knowledge.

While maintaining that carbon in steel is the great friend of the manufacturer and the only fit alloy of iron, I must admit that when it is present in quantity with phosphorus, silicon, manganese, or sulphur in quantity, it vitalizes and makes more active all of the bad qualities of these elements, and therefore if Dr. Dudley must have the quantities of these elements in his rails which he permits in his formula, he is quite right in saying that the softest rails ought to wear the best. In regard to the wearing of wire dies we have found

Carbon	Tungsten.
1.37.	0.78.

too soft, *i. e.*, it wore too easily. Also steel of the composition

Carbon,	Silicon,	Sulphur,	Manganese,	Phosphorus,
1.7	0.20	0.091	0.387	0.02

was too soft. The best foreign dies contained

Carbon,	Silicon,	Sulphur,	Manganese,	Phosphorus,
2.89	0.14	0.031	0.26	0.02

and equally good and entirely satisfactory homemade steel contained

Carbon,	Silicon,	Sulphur,	Manganese,	Phosphorus,
2.37	0.20	0.091	0.18	0.03

In this case good wear depends upon high carbon.

In steel rolls, in which we have had some experience, we have found that very soft rolls of about 0.30 carbon wore too fast from excessive flow, taking the shape shown in the accompanying cut, the overflow sometimes amounting to more than half an inch. This cuts away the brasses and involves much redressing. Rolls of about 0.70 carbon made from the same iron, neither flow nor crack to any serious extent, and will do two to three times as much work as the softer rolls.

We know that steel flows under pressure, that the milder the steel the easier it will flow, and the easier it will shear; yet in the paper under discussion flow is disregarded entirely, although there is plain evidence of it in many of the sections given. Low shearing stress is

advised as conducive to high wear, while it would seem plain that chilled flanges must act as admirable shears to trim off "flowed" edges.

Dr. Dudley says low phosphorus units ought to give the best wear, and so they ought. He proceeds to prove it by grouping the 32 rails of least wear, and the 32 rails of most wear in two groups, and taking a mean of their analyses. By a happy accident this mean sustains his view, or perhaps it would be fairer to say, from this accident he draws his conclusions. These groups consist of all sorts of rails, of different make, different conditions of wear and different tonnages. Would it not be fairer to compare rails from the same part of the track, subjected to the same conditions of wear, of the same tonnage, and presumably of the same make? Arranging the 64 rails under consideration in this way by means of the history given, I find 30 groups consisting of twos, threes, and fours. Comparing the phosphorus units and wear per million tons as given in the table, I find that in twelve groups the softer rails have shown the least wear, while in eighteen groups the harder rails show the least wear.

This gives 18 against Dr. Dudley to 12 for him, and it seems as if this mode of comparison were the fairer, if the presumption is correct, that the rails of these different groups were of the same make in each group, because in such case the chances are that the physical conditions due to modes of manipulation in manufacture, were more nearly the same than they could possibly be in the more general average given in the table.

I would like to ask Dr. Dudley to relieve us, if he can, of a term which he has introduced into his papers and specifications, and which, for the sake of harmony, ought, I think, to be obliterated. I refer to the word "hardener." This is a term properly applied to any substance contained in steel, for anything mixed with iron will make it harder. But carbon is the great "hardener," and produces all of the wonderful and useful properties in steel with which we are familiar, and of the nature of which we know so little. Carbon, then, ought to be distinguished as *the* hardener, and all other com-

ponents should be known by some other name. If not, we shall have quack steelmakers coming out with more silicon steel, phosphorous steel, sulphur steel, and the like. And why not? Are they not all hardeners, and is any one better than another.

This is a serious matter, for we have too much duplication of meanings now. For instance, if you go to a founder and ask if he has a chill to make a certain size of die or roll, he asks, in reply, how much chill you want, and you tell him, say, half an inch or an inch, as the case may be. Then he asks if you want a tough chill, or a mild chill, or a hard chill. Would not an outsider be utterly puzzled to know what a chill was?

Again a steelmaker talks of temper, and refers to steel of 0.30, 0.40, or 1.00 carbon, as it may happen; and a steel-user talks of temper, and means a yellow, or brown, or blue color, left on his steel after it is tempered. I once travelled many hundreds of miles to see about a steel trouble. A buyer had sent back a shear-knife which would not cut. Not waiting till the knife was received, I started off, and asked my partner to telegraph me his opinion after he had received the blade. We were both sure it had been burned. After my arrival on the scene and finding a man in real trouble, and a great temper, a message came in these words, "Temper too high; will send another bar." Greatly pleased, and thinking my way plain, in an evil moment I showed the message to my troubled friend, who interpreted it to mean that he had improperly treated the steel, and the indignation created was as profound as it was unexpected.

Who, then, can define steel? An International Committee of this Institute and many others have wrestled with the question, and yet to-day there is a heavy suit pending in the United States courts, all turning upon the question whether steel is steel or iron.

Now we are threatened with the war of the "hardeners," and the contemplation of another complication in our nomenclature is no joke for the steelmakers; and in their behalf I appeal to Dr. Dudley to relieve us before it is too late.

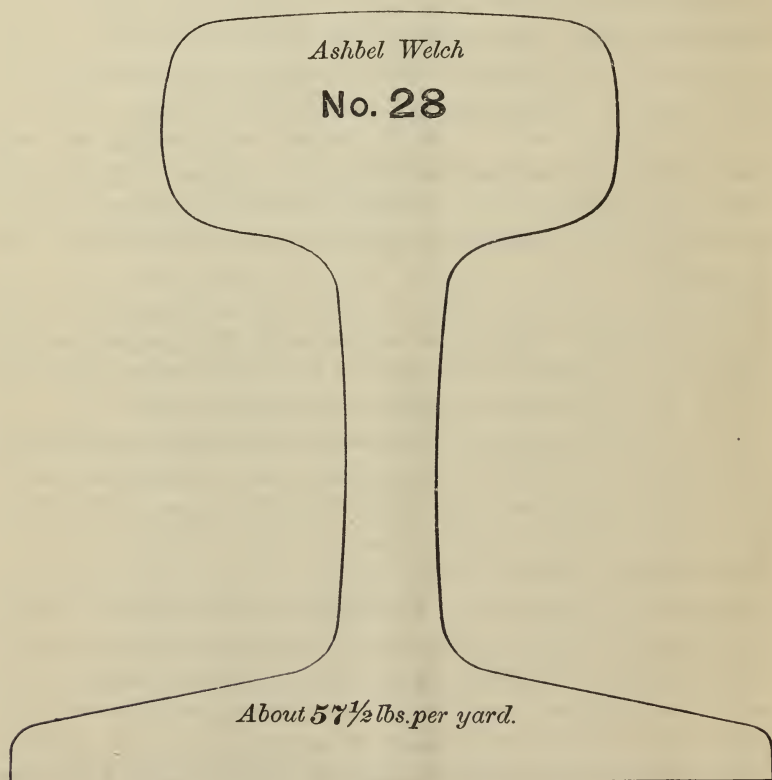
In conclusion, allow me to say that I hope my remarks will not be regarded as antagonistic to Dr. Dudley's great work, for I could have no possible reason for entering the lists as an antagonist; and, on the contrary, I decidedly agree with him that the chemistry of steel is an important factor in its manufacture, and chemistry and physics must go together if results of any value are to be reached; and I firmly believe that eventually all engineers will know how

to make and to appreciate chemical specifications. I therefore feel that we should all contribute all we know or think on the subject, and I hope these remarks will be considered a thoroughly sympathetic criticism.

LETTER FROM WILLIAM R. HART.

I was this morning an interested listener to the remarks of Mr. Ashbel Welch in regard to his designing a new section for steel rails, in 1866; and for the sake of the truth of history, and in order to give the credit to an American (and where it rightfully belongs) of designing the first section for steel rails, which was intelligently adapted to that material, I beg to state the following facts:

On the 14th of August, 1866, Mr. Ashbel Welch gave me an order for 200 tons of steel rails, to be made for the Camden and Amboy Railroad, from a section designed by him. A copy of this section I append herewith.



Messrs. John Brown & Co., of Sheffield, for whom we were then agents, at first declined to roll these rails, owing to the thinness of the flange, but subsequently accepted this order. In principle, as I think you will see from the section, this pattern was similar to what is now known as the Sandberg section, having a large amount of metal in the head, and without superfluous weight in the stem and base. This section was put upon our book, with the title "Ashbel Welch section;" and this name was also rolled upon the stem of the rails. We sold afterward large quantities of these rails under this title to the Philadelphia and Baltimore Railroad Company, as well as to the Camden and Amboy Railroad Company.

I make this statement in justice to Mr. Welch, who ought to have the credit of designing a section which had much to do with making steel rails a success. At the time Mr. Welch designed this section it was quite a new departure, and as our old section-book shows it was very different from any of the sections which up to that time had been ordered by any of the railroad companies.

I shall be very glad if this fact can be recorded upon the minutes of the Institute, and remain,

Yours very truly,

WILLIAM R. HART,

PHILADELPHIA, February 17th, 1881.

Agent, Naylor & Co.

LETTER FROM R. H. SAYRE, BETHLEHEM, PA.

. . . . The subject is one of great interest in every point of view to railroad managers and steel-rail makers. It has occurred to me that if in this connection your society would take up the matter of the shape or section of steel rails and the form of joint, and be able to arrive at such form for different weight of rails as you could recommend to the railroads of this country with a view of obtaining uniformity, it would be, in case of adoption, of great value both to the railroads and makers of rails.

My idea is that if both your society and that of the civil engineers should join in the adoption of templates and their recommendation, it would be more likely to have the desired effect.

Yours truly,

ROBERT H. SAYRE.

WILLIAM KENT, PITTSBURGH, PA.:—The steel manufacturers of this country must ever be grateful to Dr. Dudley for his painstaking and conscientious endeavor to establish the relation between the chemical analysis and the wearing capacity of steel rails. They must thank him for the vast array of facts he presents, and especially for having given them sixty-four analyses with which to combat his own conclusions and to establish their own, which are entirely opposite to his.

In Dr. Dudley's discussion of his former investigation, at the Pittsburgh meeting, he said, "If you do not like my conclusions, draw your own conclusions." I have studied his last paper as thoroughly as the limited time since I received it would admit, and have drawn some conclusions which I will first state, and then attempt to demonstrate.

Briefly stated, my conclusions are: 1st, That as far as these 64 analyses reveal anything of service to railmakers and consumers they do reveal, or seem to reveal, that within the following chemical and physical limits, viz.:

Carbon, ...	0.20 to 0.60
Phosphorus, ...	0.026 to 0.145
Silicon, ...	0.015 to 0.480
Manganese, ...	0.252 to 0.880
Phosphorous units, ...	20.3 to 5.72
Bending weight, ...	2270 lbs. to 4260 lbs.
Deflection, ...	13° to 190°

or in other words within the limits of nearly the whole range of the chemical and bending tests of these 64 rails, the wearing capacity bears no relation at all to carbon, to phosphorus, to silicon, to manganese, to phosphorus units, to bending weight or to deflection; or if there is any relation between the wearing capacity and these six or seven variables, it is so obscured by the action of other causes or variables not yet known, that such relation cannot be expressed by any practical formula.

2d. That the difference in wearing capacity of these 64 rails was not due to carbon, to phosphorus, to silicon, to manganese, or to any combination of these four elements, but that it was due to some other cause or combination of causes, of which Dr. Dudley's whole investigation furnishes us no clue whatever. A few of the many possible causes I may name, sulphur, copper, oxide of iron, inclosed air or other gases, overblowing, underblowing, overheating, underheating, too hot-finishing, too cold-finishing, cold-straightening, too great or too little reduction from the rail to the ingot, or the portion

of the ingot from which the rail was taken, as top or bottom. I have no idea which of these causes has the greatest influence in determining the wearing capacity of a rail,—probably no one else has,—but I firmly believe that some one or more of them has far more influence than all the four chemical elements named in Dr. Dudley's analyses within the limits which I have mentioned.

3d. That the railroad companies must utterly abandon, for the next ten years at least, the attempt to limit rail manufacturers to certain prescribed chemical analyses, unless within the wide range of analyses I have given above. If they would seek to establish a definite specification by which to insure good wearing capacity, and at the same time not make the specification an impracticable one for railmakers to meet, as Dr. Dudley's certainly is, they must inaugurate another investigation (and I know of no one so well fitted to undertake it as Dr. Dudley himself, after he shall have thoroughly disabused his mind of conclusions already formed), which investigation shall take at least ten years to complete, and shall include not only the effect of the four chemical elements now under discussion, but all the other supposed causes of differences in wearing capacity which I have mentioned above, besides many others I have not even thought of. Such an investigation I believe the railmakers would not object to; they should rather contribute towards it. It would richly repay the railroad companies by enabling them to secure better rails, providing that after the investigation was concluded it should reveal the causes of defective wearing capacity, which Dr. Dudley's present investigation does not do.

4th. That Dr. Dudley's failure to establish, after years of careful investigation, the relation between the chemical analysis and the wearing capacity of a rail, should be a lesson to other consumers of steel besides the railroad companies, not to attempt to regulate the steel manufacture by a chemical specification based upon their own investigations, far less elaborate than that of Dr. Dudley or that of the manufacturers. It is a common occurrence for a steel manufacturer to be asked to guarantee both a chemical and a physical specification entirely inconsistent with each other, and one or both impossible of fulfilment. The attempt to fill orders with such specifications is not only annoying but often even ruinous to the manufacturer, causing the rejection of much steel that is even better adapted to the wants of the consumer than some of the steel which is accepted. This is precisely the result which would happen if Dr. Dudley's rail specifications were to be insisted on.

I will now undertake to give a reason for the conclusions I have drawn. Suppose that the Pennsylvania Railroad Company were at once to insist that all rails furnished them should conform to Dr. Dudley's specifications in these six particulars:

Carbon,			.25 to .35
Phosphorus,			not over .10
Silicon,			not over .04
Manganese,			.30 to .40
Bending weight,			not over 3000 lb
Deflection,			not under 130°

Suppose that before the rail manufacturers had "got the hang" of making rails within these rigid limits, and while they were yet making them with the wide range of composition which they are now doing, I should be sent as inspector to one or more works to inspect 64 lots of rails, one rail being taken out of each lot for test, the test rail being supposed to represent exactly the lot from which it was taken. Suppose, further, that I test these 64 rails chemically and physically, and find that the results are exactly those given in Dr. Dudley's tables. I tabulate the results as I have done here, rejecting all the tension and torsion tests, as these are not included in my instructions, and I carefully examine the table to see how many lots I should accept and how many I should reject. How many of these 64 rails or lots would I have to accept under these specifications? *Only three!* And one of these three would be classed by Dr. Dudley as a bad rail, standing number 10 out of the 16 rails marked "Level Tangent."

I would reject *sixty-one* of these sixty-four rails (or lots), for violation of from one to all six of the specifications.

34	for too great bending load.
29	" small deflection.
42	" high or too low carbon.
31	" high phosphorus.
32	" high silicon.
52	" high or too low manganese.

As Dr. Dudley classes the 64 rails into two grand divisions, viz., 32 slower wearing and 32 faster wearing (I call them here good rails and bad rails for the sake of brevity), I have to reject 30 of his *good rails*.

13	for too great bending load.
7	" small deflection.
21	" high or too low carbon.
11	" high phosphorus.
14	" high silicon.
21	" high or too low manganese.

On the tables (Plates 6 and 7) I have marked each cause of rejection with a black mark. You will notice that they seem covered all over with black marks, that their position follows no regular law, but that I seem to have distributed them with commendable impartiality. I count the black marks, and find the total causes of rejection to number 210 out of a possible 366, or an average of 3.44 for each of the 61 rails rejected. Of the 31 bad rails rejected, the black marks or causes of rejection number 123, an average of 3.97. Of the 30 good rails rejected, the causes number 87, an average of 2.9. Here is a point in favor of Dr. Dudley, the 31 bad rails rejected do have a worse chemical and physical record than the 30 good rails rejected, the average number of black marks against the bad rails being greater in the ratio of 3.97 to 2.9. But his gratification in this regard must be somewhat diminished when he learns that by selecting a number of very, *very* bad rails, which I have done on the tables, drawing heavy lines around them and marking them *worst rails*, viz., the 2 worst rails out of 16 marked tangent grade, the 4 worst rails out of 8 marked curve grade, low side, the 3 worst out of 8 curve grade, high side, the 3 worst out of 16 level tangent, the 2 worst out of 8 level curve, low side, and the 4 worst out of 8 level curve, high side, the total causes of rejection of these 18 rails is 68, or an average of only 3.78.* That is, the 18 worst rails have fewer average causes of rejection than the whole 31 rejected rails designated "faster wearing," in the ratio of 3.78 to 3.97.

In Dr. Dudley's table of averages he compares the chemical and physical properties of 32 slower-wearing rails with those of 32 faster-wearing rails in all conditions of service. The comparison agrees with his conclusion except in the matter of silicon, the average silicon of the bad rails being lower than that of the good rails. Let us carry the comparison a little further, and compare the properties of the 18 worst rails, which I call very, very bad rails, with those of Dr. Dudley's 32 bad rails, and see whether it strengthens his position. Here is the comparison :

	Loss per million tons.	Bending weight.	Deflec- tion.	C.	P.	Si.	Mn.	Phos. units.
32 good rails,	.0506	2878	160°	.334	.077	.060	.491	31.3
32 bad rails,	.1028	3222	133°	.390	.106	.047	.647	38.9
18 worst rails,	.1326	3209	135°	.412	.109	.040	.677	40.4
13 bad (?) rails,	.0668	3267	136°	.369	.105	.050	.632	37.9

* The method of selection of these 18 rails is not an arbitrary one, as will be seen further on. Of the 16 level tangent rails, 14 should be classed as good and 2 as bad; a fairer classification than that of 8 faster and 8 slower wearing.

The 18 worst rails show a greater loss per million tons than the 32 bad rails by 29 per cent., while in three elements of Dr. Dudley's specifications, viz., bending weight, deflection, and silicon, they are slightly better, according to his ideas, and in three others, viz., carbon, phosphorus, and manganese, slightly worse. If Dr. Dudley's conclusions were correct we should expect to find that a lot of rails having 29 per cent. worse-wearing capacity than a larger lot which included them, would show a very marked deviation in average analysis and bending tests from the average analysis and bending tests of the larger lot; but actually the deviation is so slight that Dr. Dudley himself could not tell, if the results of tests alone were presented to him, which lot had the greater and which the less wearing capacity.

But I have carried the comparison still further. In the fourth line in the table I have placed the record of the other 13 bad (?) rails, out of the 31 faster-wearing rails, which would have been rejected under Dr. Dudley's specifications. (I have omitted in this average, rail No. 920, classed by Dr. Dudley as a bad rail from its faster wearing, but which conforms to his specifications in every respect.) I have placed an "(?)" after the word bad in designating these rails, as I think they should have been classed with the 32 good rails, on account of their wearing capacity being much nearer to that of the good rails than to that of the 18 worst rails with which they are associated. Comparing the 18 worst rails with the 13 bad (?) rails, we find the former to be over 98 per cent. (or nearly double) worse in wearing capacity than the former, while their analyses and bending tests indicate them to be very nearly alike. The worst rails are lower in bending weight and silicon, and only slightly lower in deflection, and slightly higher in carbon, phosphorus, and manganese. Here is a very plain case selected from Dr. Dudley's own work. Two distinct lots of rails, one lot twice as good in wearing capacity as another, but both lots closely agreeing in average analysis and bending tests. What stronger evidence could be produced of the fallacy of his formula?

If, in any large series of observations of the values of different variable quantities, such as are here dealt with, there exists any law of interdependence of such variables, the figures representing such observations can be plotted on cross-section paper, and the law will be plainly revealed by a curve or straight line drawn through the various observations. I have thus plotted the results of Dr. Dudley, and have made 42 curves, or attempts at curves, to discover, if pos-

sibe, the existence of any law of interdependence of the variable wearing capacity in the six series of rails, viz., tangent grade, curve grade high side, curve grade low side, tangent level, level curve, high side and level curve, low side, with the seven other variables, bending weight, deflection, carbon, silicon, phosphorus, manganese, and phosphorus units. Here is the diagram (Plate I) with the forty-two attempts to form as many curves. You will see there is neither curve nor straight line here,—nothing but a heterogeneous mass of ups and downs and straights across,—not the slightest indication of any law in any single curve which is not contradicted by another curve of the same variable in another series.

This set of curves, or rather zigzags, shows plainly my reason for separating Dr. Dudley's 32 bad rails into two series, one of 18 worst rails, the other of 14 which nearly approach the good rails. In the curves of the tangent grade rails, the two worst rails, 882 and 893, in their plotted positions in the curve remove themselves far from the main body of these rails, and therefore in contrast with the others they are justly named the very, very bad rails of this lot. This grouping of the very bad rails far away from the better rails is still more plainly seen in the level tangent rails, where 13 rails are comparatively near together in position, and the other 3 are far removed from them, thus plainly indicating that if we wish to consider the relation of the chemical composition of these 16 rails to their wearing capacity, the method of averaging the 8 faster wearing and the 8 slower wearing—which Dr. Dudley seems to think is the only good one—is not the best by any means, but that to obtain an intelligent deduction the thirteen fairly good rails must be contrasted with the three rails which are widely different from them in wearing capacity.

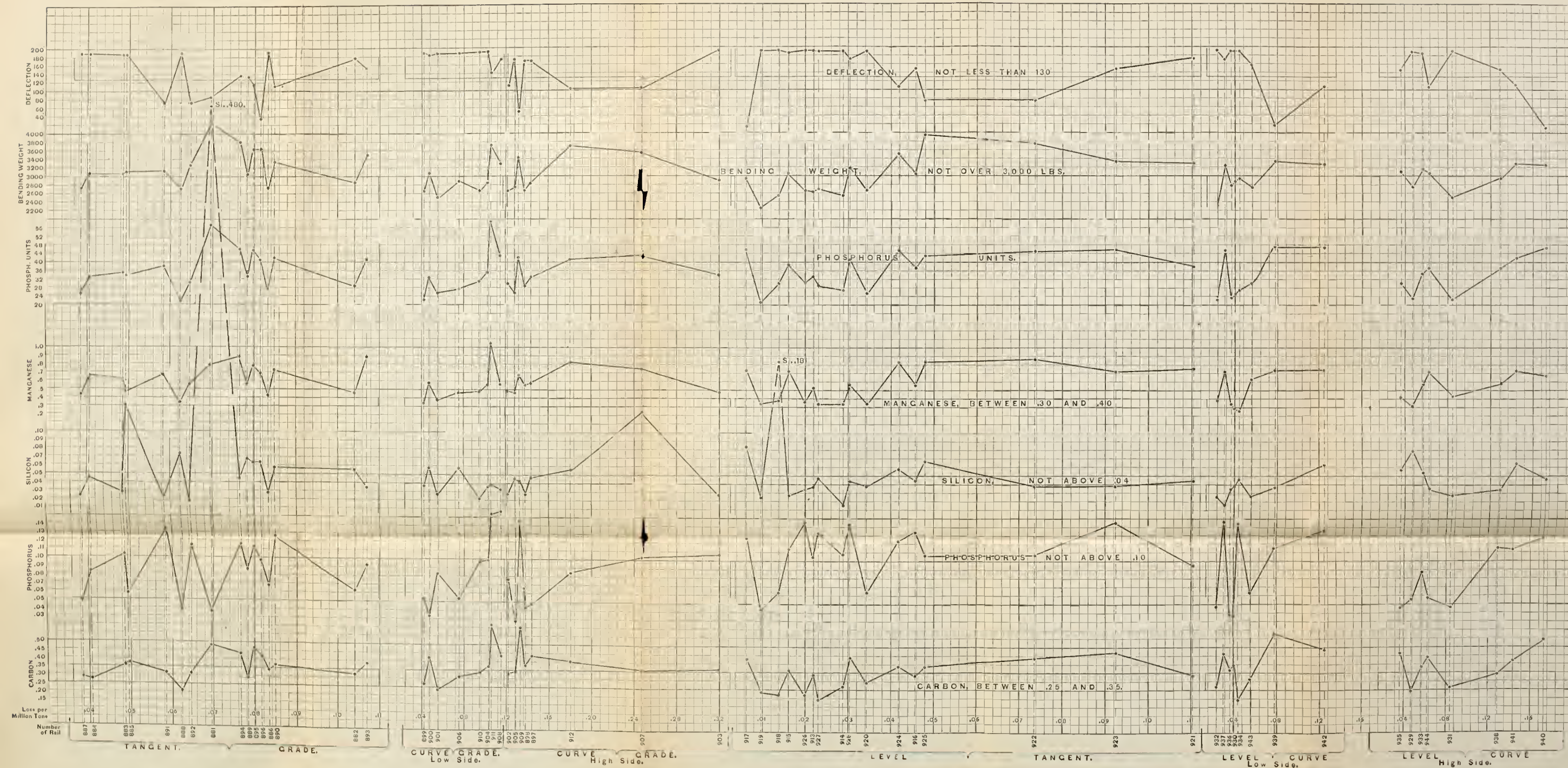
I have said that these zigzags indicate the absence of any law of relation between wearing capacity and the six variable quantities under consideration. I am surprised that Dr. Dudley did not include his phosphorus units in his specification, as I regard this idea of phosphorus units as a most valuable conception, and one likely to lead to the advancement of our knowledge of the relation of the physical to the chemical qualities of steel. For this reason I have included phosphorus units in my plate of zigzags, although Dr. Dudley has not named it in his specifications. I thought it probable that by plotting the phosphorus units, I might possibly discover whether they had any relation to wearing capacity, although carbon, phosphorus, silicon, and manganese had no such relation. I discovered none. But there is a very marked peculiarity in the zigzags repre-

senting phosphorus units besides that of general relation to the four chemical elements which follow as a matter of necessity, namely, the almost entire parallelism of the lines of phosphorus units and bending weight. This suggests at once the existence of a relation between phosphorus units and bending weight, and I have, therefore, made another diagram, plotting the figures expressing this relation. The accompanying Plate II shows bending weight at the side of the section paper, and phosphorus units at the bottom. You will plainly notice that there is an evident trend of the dots representing the rails in the direction of an ascending inclined straight line. The whole range of bending weights being 2000 pounds, the deviation of the position of any rail from this inclined straight line (with only two exceptions) is an ordinate representing less than 450 pounds. The inclined straight line is the average direction of trend of these dots, and the individual dots do not deviate greatly from this average direction. The law of the relation between phosphorus units and bending weight is thus clearly established, and it is this: *The increase of bending weight is directly proportional to the increase of phosphorus units.* The discovery of the law is a strong presumptive proof of the correctness of Dr. Dudley's hypothesis in regard to the hardening influence of the four chemical elements upon steel, viz., that the relations of phosphorus, silicon, carbon, and manganese are to each other in respect to hardening as the numbers, 1, $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{5}$. Dr. Dudley is certainly to be congratulated upon this discovery of presumptive proof of his hypothesis.

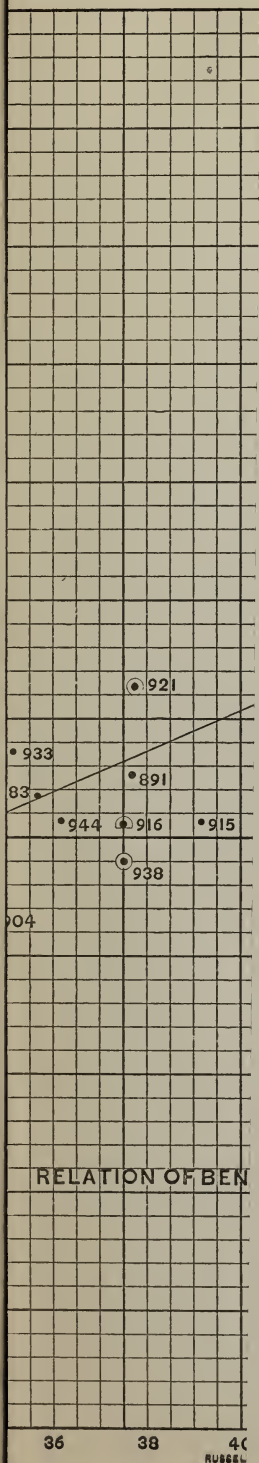
The fact of the law of relation of bending weight to phosphorus units being so plainly indicated in these 64 analyses and tests, while there is no such indication from these tests of any similar relation existing between wearing capacity and phosphorus units, or between wearing capacity and carbon, phosphorus, silicon, manganese, bending weight, or deflection, is almost absolute proof in itself that such relation does not exist at all, or, as stated in the first part of my remarks, if it does exist it is entirely obscured by the influence of other variable quantities not considered in Dr. Dudley's paper.

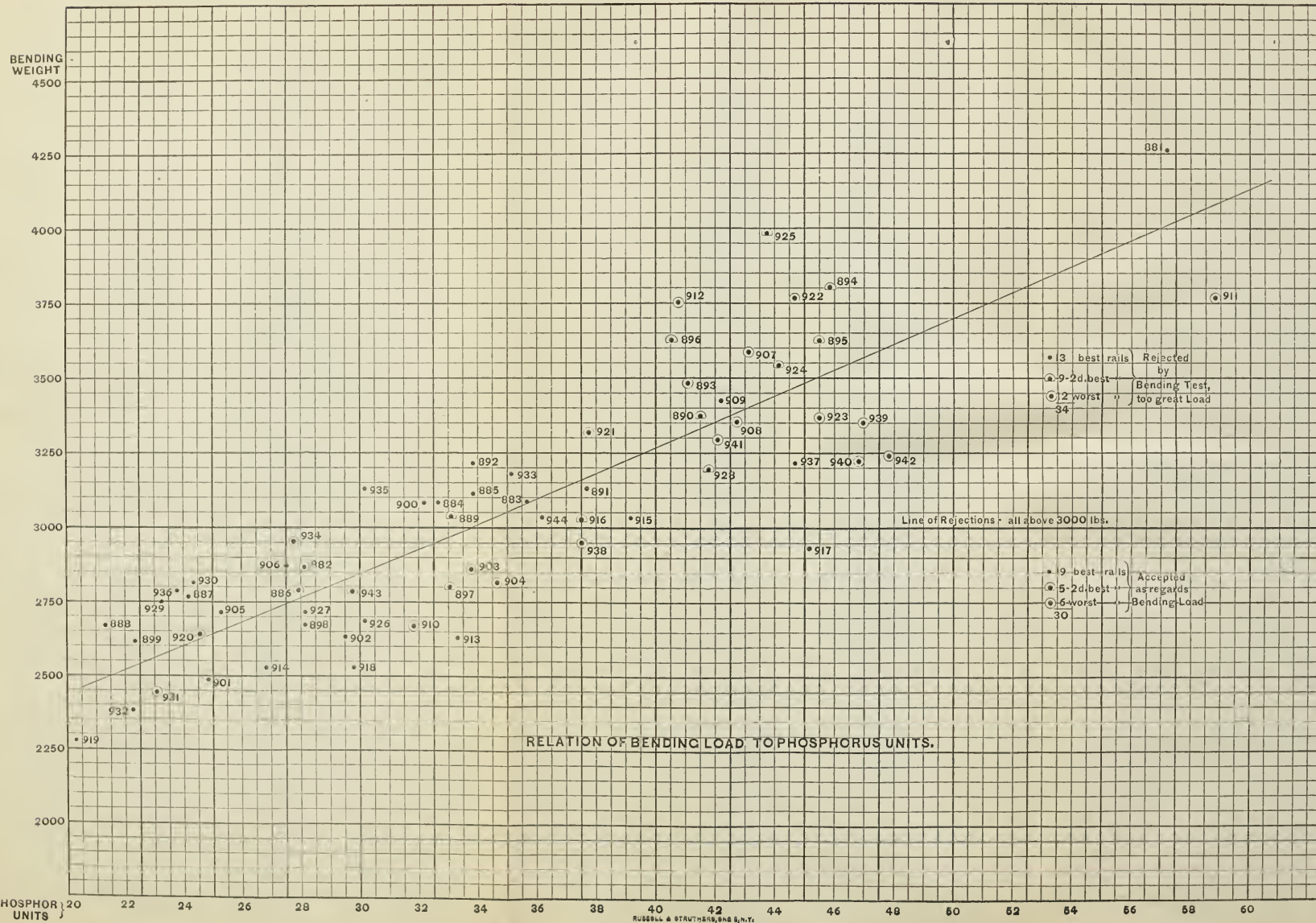
On the diagram on which is plotted the relation of phosphorus units to bending weight I have indicated the position of each of Dr. Dudley's 32 slower-wearing rails by a single dot. The 18 worst rails of the 64 are indicated by a dot surrounded by a circle, and the remaining 14, called second-best on the diagram, are indicated by a dot surrounded by a semicircle. By drawing the line between rails which would be rejected on Dr. Dudley's specification for bending





Mining Engine





weight alone, viz., not above 3000 pounds, and counting those above the line, it is seen that there are 34 rails rejected out of the 64, and of these 34, 13 are the best rails of the series (Dr. Dudley's slower-wearing rails), 9 are the second-best rails,—almost as good as the former,—and 12 are from the 18 worst rails. Counting the accepted rails, a total of 30, 19 are best rails, 5 are second-best, and 6 worst. As already shown, of these 30 rails, which might be accepted on the ground of their bending weight being below 3000 pounds, only three of them could pass the gauntlet of all the other five specifications, and one of these three is a second-best rail.

I think I have now proved my first proposition, but I must anticipate an objection which Dr. Dudley may possibly raise. He may say that my argument against his formula, based upon the rejection of 61 rails out of 64, is invalid, because these rails were not made according to his formula. Let them be made according to my formula, he may say, and they will not be rejected, and they will give good service. I regret that he has given us the record of only three rails which do conform to his formula; they are not sufficient in number to draw valid conclusions from, but, such as they are, here is the conclusion they lead to. Two of them are slower-wearing, one a faster-wearing rail; therefore, out of three rails which conform to the formula, the chances are that one rail will not be a good one. Are the railroad companies satisfied with such a result as this? But suppose, for the purpose of admitting a greater number of rails into our accepted list, we relax the specifications slightly. We will say that rails must be made according to Dr. Dudley's formula, but if a rail happens to conform to five of the six specifications and fails in only one, and that one manganese, the least injurious element considered (Dr. Dudley himself considering it only one-fifth as bad as phosphorus), then we will not reject that rail. Our list of accepted rails will then read as follows:

No.	Service.	Order in Dr. D.'s table.	Max. load.	Deflec- tion.	C.	P.	Si.	Mn.	P. units.
920	Level tangent	10th in 16	2630	190°	.293	.063	.039	.326	24.6
932	" curve, low side,	1st " 8	2340	190°	.269	.047	.026	.372	22.4
943	" " "	6th " 8	2780	159½°	.314	.061	.025	.602	29.8
931	" " high side,	5th " 8	2430	190°	.260	.047	.029	.416	23.1
887	Tangent grade,	1st " 16	2770	190°	.287	.048	.023	.435	24.2
886	" " "	13th " 16	2790	190°	.349	.069	.026	.404	27.9
899	Curve gr., low side,	1st " 8	2620	190°	.263	.051	.038	.326	22.3
910	" " "	5th " 8	2660	190°	.343	.098	.020	.478	31.8

Here are eight rails, unquestionably the softest rails of the series,

conforming exactly to Dr. Dudley's specifications, except four which have manganese less than one-tenth of a per cent. higher, and one with manganese two-tenths of a per cent. higher than the specifications. We ought to expect that all or nearly all of these eight rails would be included in the thirty-two rails designated by Dr. Dudley as slower wearing, but, on the contrary, there are only three of them so included, and five of them are the faster-wearing rails. It has been said that exceptions prove the rule, but there is such a thing as having more exceptions than rule, and I think we have such a case before us.

After Dr. Dudley has spent some years investigating the difficult problem of determining what physical or chemical properties have an influence upon wear, he tabulates his results; he studies them carefully; he then proceeds to write his report and to draw his conclusions. In the report we find the following words:

"Giving our attention now to the tables, I think the first observation will be that there is no absolute gradation in physical qualities, or in chemical composition, applying to every rail in each group, which corresponds to the gradation in amount of metal lost per million tons."

Then he tells us we ought not to expect such uniformity, for two reasons:

1st. Errors in determining loss of metal and tonnage.

2d. I quote his own words: "I am not aware that it is known as yet exactly what wear is, or what it is dependent upon; . . . whether wear is a direct function of the tensile strength of steel, or of its elongation, or of its elastic limit, or of its resilience, or any combination of these, or indeed, as seems somewhat probable, of the amount of distortion by bending that a piece of steel will suffer, is a problem yet to be solved."

It certainly is a problem *yet to be solved*, and it will take many years to solve it. Dr. Dudley should have stopped here, and drawn the conclusion which I have drawn, namely, that, within the wide range of analyses and bending tests which I mentioned in the first part of these remarks, as far as these sixty-four tests show anything, they indicate that whatever wear is or may be dependent upon it is not dependent upon carbon, phosphorus, silicon, manganese, bending weight, and deflection. Instead of this, in one breath he admits he does not know what wear is dependent upon, and in the next he formulates the extraordinary *non sequitur* that it is dependent upon carbon, phosphorus, silicon, manganese, bending

weight, and deflection, and recommends that the Pennsylvania Railroad Company demand that rails be made on specifications, based on these six variables, so narrow that to fill them would cause the constant rejection of enormous quantities of steel, and a consequent enhancement of the price of rails, probably ten or twenty per cent., without any certainty that such rails would be any better than those the steel-mills are now making.

I earnestly hope that the investigation which Dr. Dudley has so ably carried on will be continued. I hope the Pennsylvania Railroad Company, or preferably a combination of several railroads, will institute the prolonged investigation which I think will be necessary to solve this deep problem; that they will take a hundred or more rails, watching and noting down carefully every detail of their manufacture as well as their analysis; that they will be carefully weighed before, during, and after service; that their crop-ends will be tested before service, and the rails themselves after removal; that all the sources of error which Dr. Dudley admits in the present investigation may be removed, and that enough facts may be gathered and tabulated so that the conclusions which may be drawn from them may be apparent to every one without labored discussion or heated argument. But I venture to prophesy that after this investigation shall have been completed, and a formula adopted which shall be satisfactory to both manufacturers and consumers, that formula will not be the one now under discussion.

I hope Dr. Dudley will pardon me if I have been unduly severe in my criticism, and consider that I have written my remarks hastily and at a time which should have been given to needed rest. I differ with him only as regards the conclusions which he has drawn. I appreciate the value of his labors, and only make public my own conclusions in the hope of contributing to the advancement of the science of steelmaking, in which we are all so deeply interested.

DR. AUGUST WENDEL, TROY, N. Y.: Dr. Dudley's last paper gives, certainly, very valuable and interesting information regarding the wear of rails under different conditions. His results concerning the composition of rails, explode, rather startlingly, some old theories regarding the wear of rails, and I think after his formula is simplified it will be *one* good formula to work by.

As he now arrives at the same conclusions reached in his first paper, some of my remarks will apply to both, although I would

not attempt to add anything directly to the distinguished criticism the first paper received.

Regarding his silicon percentages, I must say I cannot see any reason why they have not been disregarded entirely within the limits of his investigation. In following Dr. Dudley's reasoning in the use of averages and applying inductive methods, I cannot see what importance can be attached to this element, since, in his first investigations, the good and bad rails averaged nearly alike, and in his last series the rails which wore best, showed even more silicon than the bad ones. I regard, therefore, the silicon an inconsiderable factor in making out the phosphorus units, without considering here the actual influence of this element on hardness,—an influence which is greatly overestimated in the formula.

With regard to the effect of manganese, I cannot agree with his conclusions. In ordinary working with full-blown steel, manganese is more or less the function of carbon, provided the spiegel is constant, and consequently should not be introduced into a formula for daily working as an independent coefficient.

In March, 1875, I made a report to my employers concerning the unsatisfactory working of steel in blooming.* I then came to the conclusion that steel ingots, in order to roll well ought to contain :

$$\text{Mn} = 0.8 (\text{C} + \frac{1}{2} \text{Si}) + 4 \text{P},$$

these symbols standing for the respective percentages of the elements. I maintain to-day, that for good results in blooming, this percentage of manganese ought to be aimed at for rail steel. For this reason I would sooner undertake to make steel according to Dr. Dudley's original formula, viz.:

Carbon, ...	0.334
Silicon, ...	0.060
Phosphorus, ...	0.077
Manganese, ...	0.491

than according to the one in which he tries to make concessions to the manufacturers, viz.:

Carbon, ...	0.30
Silicon, ...	0.04
Phosphorus, ...	0.10
Manganese, ...	0.35

* Transactions, Vol. IV, page 364.

Now in spite of all the progress in steel manufacture, we have not succeeded in making up steel by prescription, and what would therefore become of the ingots in which manganese for some cause or other should happen to fall below Dr. Dudley's meagre allowance? The answer will be: "Works must return to the old practice of hammering ingots," but I doubt very much whether even by hammering a sounder bloom, and as a consequence, a better rail is obtained.

German government officials who as a rule are not happy unless they can make things unpleasant for somebody, must have got hold of Dr. Dudley's formula, as they lately insist upon steel being hammered. Now, there is not the least doubt that some of the steel under Dr. Dudley's investigation was hammered, but I do not deem it necessary to resume that practice, if a larger manganese percentage is used.

I am greatly encouraged in this statement by the analysis of the *rara avis* of the series. I refer to the one showing 0.48 silicon. I would consider it sound metal, since it satisfies my equation regarding manganese, and still, in spite of its increased hardness and its enormous phosphorus units, is satisfactory in wearing capacity.

The conclusions of Dr. Dudley's ingenious experiments seem more simple than he cares to admit, and could be condensed by simply saying: "Use iron low in phosphorus, and do not make the steel too hard."

Regarding tests of steel made from such iron, I would even be more stringent than Mr. Sandberg. Of each blow I would make a bar about one inch square, and bend it cold. It should not be so hard as to resist bending to the shape of a horseshoe, nor should it be so soft as to bend 180 degrees without showing signs of fracture. There would thus be obtained a quality of steel that would more than satisfy Dr. Dudley's pretty theory of infinitesimal teeth by creating those whose tendency would be to neither flatten nor to bend. I think that in making this test, and supplementing it by the carbon test, manganese and silicon would regulate themselves much more nicely than any specification could effect. Every steel maker knows that, should the silicon run high, the heat is blown too short, carbon will be increased considerably, and the test will not stand. On the other hand, if manganese is high, the heat has received too much spiegel (and that is simple awkwardness) and carbon would show the same result as above.

In conclusion, I should think that railroad authorities, under all

circumstances, would prefer the steel with which they are now familiar, to a specimen that Mr. Sandberg has described as having broken into seventeen pieces under the wheels. After blowing such low manganese steel, it may be coaxed into a rail, and it is a wonder that it holds together so long as it does, with so great a number of minute flaws.

I would not in any way depreciate chemistry, but I think it should be kept in its proper sphere. Let the chemist look after the quality of pig metal, and apply common sense in the avoiding of extremes, then the most fastidious railroad cannot find fault with the result.

PROFESSOR EGGLESTON, NEW YORK CITY : It is not my place as an engineer to apologize for the chemists, but as no one seems disposed to do so, and as they have had more than their share of criticism, I am glad to say that I believe there are chemists in this Institute whose work and word is just as reliable, and perhaps even more so, than that of the average engineer. But we ought to make a distinction ; there are chemists and chemists. With the ordinary commercial chemist, who looks upon the science as so much merchandise, I have not a particle of sympathy ; but with the chemist who looks upon his profession as engineers do upon theirs, I have every sympathy. When manufacturers and engineers go to a chemist and ask him to make an investigation, and screw him down to the lowest point, turning the equivalent of his brains into cents and mills, they usually get an exact equivalent in poor work for miserable pay, and no one has or should have any sympathy with them, and the manufacturer under these conditions has no right to ask for any, and no reason to criticize any work that he may get under such circumstances. But I am disposed to think that the chemists who have been represented and discussed at this meeting do their work conscientiously, and that it is as reliable as that of almost any profession.

I believe, however, that this problem of steel rails is being investigated in a wrong direction. I said so at the Pittsburgh meeting, and I think the discussion of this meeting will prove it to all those who have heard it. I think the chemist is incapable of solving this problem unless he goes very far into the domain of physical chemistry, so far that it becomes physics and not chemistry ; and that the physicist will be the one on whom we must rely in the future for the elucidation of the subject. The chemist may aid the physicist, but it is my decided opinion that we are looking in the wrong place to get the explanation of the phenomena.

Attention was called at the St. Louis, and afterwards at the Baltimore meeting, to the fact that the pounding which a rail receives from the falling of the engine from a high rail to a low one was sufficient in many cases to account for effects which have not been explained in this discussion. This statement was made after an extended observation had been made of rails laid over many hundred miles of railway in Europe. But if it is true, as Mr. Cloud stated in his papers read yesterday, that the blow of the engine is repeated not only at the end of the rail, but every time the driving-wheel makes a revolution, it will explain much of the giving-way of the rails over their whole length, and the effects of these blows on the physical condition of the steel should be very carefully investigated. It was shown in the discussion of the law of fatigue and refreshment of metals at the meeting in Montreal, that every blow was accompanied by a physical effect which could be rendered distinctly visible. The blow and pressure of the gag which always leaves its trace on the rail is certainly less of a physical effect than the constant and rapidly repeated series of blows which the rail receives from the continual passing of trains, yet the gagging always injures a rail and sometimes destroys it.

At the Baltimore meeting two rails were shown which had been placed in essentially different conditions, and which had been subject to a cold flow of the metal in every part of the rail even to the very outside of the foot. Mr. Metcalf yesterday showed this same kind of a flow in rolls. He also spoke of the ignoring of the copper in the analyses of steel rails. At the Washington meeting in 1876, the statement was challenged that good steel rails had been made containing any very large percentage of copper, and though repeatedly promised the analyses of such rails they have never been produced. Some years ago, in visiting one of the largest steel works where the ingots are compressed, I noticed a jet of blue flame passing out from the bottom of the ingot mould, which I at first thought was phosphorus, but which I afterwards determined to be copper. Certainly, if there is enough copper in the rail to allow of its becoming visible in the color of the flame, there must be enough to influence its physical condition and its life, and we cannot afford to neglect it.

I have also had occasion to show that bubbles produced in the steel ingots were never rolled out of them, and that if they were once in the ingot they remain in the rail and arrange themselves in such conditions that they were sure sooner or later to engender

a weakness in the direction which the cavities take. As these bubbles are never rolled out as the rail wears, they come to the surface, and the rail which shows a chemical analysis which is perfect, becomes imperfect from a physical defect. Every one who is familiar with the breaking of metals in a testing machine has been made aware of the fact that these bubbles will often cause metals whose analysis is faultless, to break with a very low tensile strength, while a piece without bubbles, taken from the same sample, will break with a high one. These bubbles are sometimes finer than pin-holes. I have, in investigating this fact in railroad material, occasionally come across iron and steel which were so fatigued at the time that they left the rolls, that they were really unfit for service; and I have seen new rails which I should not feel justified in placing in any other position than on a side track. This question of fatigue in the course of manufacture has not been discussed at all so far as I know. I took occasion to show in the St. Louis meeting, that a rail taken from the Northern Railway of France, which I had the pleasure of examining, and which was condemned as good for nothing, and which the manufacturers were obliged to replace, when brought to Paris and submitted to chemical and physical tests, proved to be as good as any rails which were in the service. These kind of physical defects are the ones which are to be looked after; and, in fact, nearly all the defects for which a chemical solution is demanded are physical. I have within the last year had occasion to examine metals and alloys which are fatigued, and find that while chemically the same, they act physically so entirely differently that I hope at some future meeting of the Institute to bring the matter before you.

I am at a loss to know why certain chemical substances which we know are in the iron or are likely to be there, should not be determined, and their effects discussed. Copper is one of these, sulphur is another, titanium and vanadium are known to be sometimes there. Why should not the effects of these substances be discussed?

Mr. Metcalf has alluded to the question of occluded gases. I have had occasion to show recently in my laboratory that as a general thing metals and alloys which were brittle from fatigue, contained a much larger amount of gas than the same metal which was not fatigued. I have also ascertained that the metals in this condition go into solution in a manner quite different from those not fatigued. I think also that the place from which the sample is taken from the rail will make some difference with regard to the results. Some one, I do not remember who, made an investigation some

years ago upon the effect of having the rails always rolled in the same direction, and also of having them rolled backwards and forwards, and showed that under the latter course there were of necessity weak spots somewhere near the centre of the rail, yet in all this discussion the methods of rolling have been passed by almost in silence.

Mr. Sandberg in his paper mentions the idea of using a registering dynamometer attached to the punching machines, and of determining the quality of the metal by the resistance which is shown. I think the first idea of this kind was published by Professor Langley, then of Pittsburgh, who, while making some investigations for Messrs. Miller, Metcalf and Parkin, announced as the result of a series of dynamometric experiments that abrasive resistance was the term which should be used in regard to steels of different wearing qualities. I have had extremely delicate dynamometers attached to the instruments of precision with which I am making the investigation on the fatigue of metals, and hope soon to communicate the results of the investigation made with them to the Institute.

I wish again to call attention to the fact that we are using the words "hardener" and "hardness" without any real idea of the meaning of these words. When we say hard and soft, as we have been constantly doing during this discussion, is it quite sure that every one has exactly the same meaning in his mind? Certainly, when hard is used in distinction from soft, we mean not the capacity of wear, but the capacity of resistance to penetration, to fracture, or some other resistance, and do not always mean the capacity of resistance to abrasion or crushing, which the discussion would sometimes seem to imply were the only qualities requisite to constitute a good rail.

J. W. CLOUD, ALTOONA, PA.: I would call the attention of the Institute to the title of Dr. Dudley's interesting and valuable paper,—"The Wearing Capacity of Steel Rails in relation to their Chemical Composition and Physical Properties." Here are two separate and distinct questions: 1st, The wearing capacity of steel rails in relation to their chemical content; and 2d, the wearing capacity of steel rails in relation to their physical properties.

The discussion has been almost exclusively confined to the former question, on which there may be many differences of opinion in matters of detail without greatly affecting the result. The latter

question has been ignored, and I wish to call attention to it, particularly, because it is the practical question after all from the consumer's standpoint, and because the paper under discussion is more decisive on this question than on the other. In fact, I think we must admit that Dr. Dudley has established his main point, viz., that the softer steel, physically, gives the slower wear, contrary to general belief among engineers in this country.

Of course, we are all interested in the whole subject from a scientific standpoint, but as the discussion has been participated in largely by manufacturers of steel, with a business animus, and as they have endeavored to overthrow Dr. Dudley's chemical recommendations because they are necessarily the most vulnerable points in the paper, I wish to recall attention to the facts of the physical properties as being the consumers only practical guide, and as affording the most conclusive results in the data before us.

The consumer of steel rails cannot test every blow chemically, but he can do so physically, and it is my opinion that a bending test of a whole rail section, say three feet long, under steady pressure, instead of a drop, with a specified deflection, without cracking, and a load not to be exceeded to produce this deflection for each rail section, will be a good practical test, not burdensome to manufacturers, and one that will insure consumers such a degree of softness in steel as they may desire.

They should not attempt to dictate to manufacturers how this degree of softness shall be obtained chemically, but allow them full freedom to do as they please, so the proper physical properties are had. Further, we are now in possession of the information requisite to make such specifications with the certainty of greater safety as well as more economical results in the life of rails.

I have recently had opportunities at Altoona, to see other and very convincing evidences of wear, as compared with physical softness. The locomotive driving-wheels probably cause at least one-half of the wear of rails, and the forces which go to wear the rails from the tires, must react with similar intensities on the tires themselves. We therefore have in driving-wheel tires, an opportunity to see the wear in a more concentrated form, so to speak, or with rates as well as differences magnified. I have recently found differences of one inch to two inches in circumference of two tires on the same axle when coming to shop for turning, and it is invariably evident that the smaller tire is much the harder, the chips from it being short and brittle, while chips from the larger tire are much

longer and tougher. In the worst case I have observed, viz., two inches difference in circumference, this difference in hardness, as observed from the cutting, was more marked than in the other cases. Tires are always grouped in sets by the manufacturers from their knowledge of the chemical composition of the steel, with an attempt to get those in one set which have the same degree of hardness, so that the wear shall be equal all around; they succeed pretty well on the average, but I have been noting the exceptions.

JACOB REESE, PITTSBURGH, PA.: I have been very much interested in the reading and discussion of Dr. Dudley's paper. As far as it relates to the data of work performed by the rails, and the determination of their physical and chemical properties, I have nothing but commendation of Dr. Dudley to express, as the investigation covered a greater range, and was performed with more care in detail, than any similar work which has come under my notice. But I beg leave to differ with Dr. Dudley in his conclusions.

What are the factors of hardness? Are they not carbon, silicon, phosphorus, and manganese? Now it is an undisputed fact among metallurgical experts that pure carbon and pure iron make the best steel of all degrees of carburization, and for all purposes. While carbon hardens, it also strengthens the metal, but silicon, phosphorus, and manganese, in hardening, make the metal also brittle, and are injurious in any amount. Carbon should be called a *strengthenener*; and I claim that a steel rail made hard with carbon, with the other three hardeners absent or reduced to a minimum, will carry a greater tonnage than any of Dr. Dudley's soft rails.

But until the basic process is put into operation in this country we cannot expect to produce Bessemer or open-hearth steel without the presence of silicon, phosphorus, and manganese, in considerable quantities, and I greatly doubt the possibility of reducing the percentage of any of them by the present practice without seriously diminishing the output, and correspondingly increasing the net cost of production; which is an important question, since the increased life of the rail may be more than balanced by its increased cost.

I think that the soft rails performed a greater amount of work, because they contained a less amount of silicon, phosphorus, and manganese (*brittlers*, if I may so term them), and that carbon does not reduce the wearing capacity of rails. I believe that a rail made by the basic process, with silicon, phosphorus, and manganese reduced to a minimum, and containing 0.60 carbon, will be stronger

and tougher, and will carry double the tonnage of any of Dr. Dudley's soft rails.

C. E. STAFFORD, STEELTON, PA.: I must confess my high appreciation of Dr. Dudley's conscientious and painstaking work, and of his scientific methods in obtaining the data; but with his method of handling these results and with his conclusions drawn therefrom I cannot agree. The reasons for this difference of opinion I will endeavor to explain.

It is apparent on inspecting his Plates 6 and 7 that the majority of the slower-wearing rails are from the north track, and generally have a longer "time of service," a greater average tonnage per rail, and a smaller average tonnage per year per rail than the faster-wearing, the majority of which are from the south track, and generally have a shorter "time of service," a smaller average tonnage per rail, and a greater average tonnage per year per rail. Have these facts any significance? Have these differences of conditions to which they are subjected any bearing on the relative wearing capacity of these rails? I venture to say they have. I think, after a study of Table I (page 47), (an arrangement of lines 17 and 18, Plate 8), in connection with Plates 6 and 7, we will find that the slower wear of the 32 best rails is only partly due to qualities inherent in the rails themselves, but is principally due to external conditions favorable to slower wear.

In regard to the north and south track, we know that over the south track come the loaded cars from the west, and that over the north track these cars return, most of them empty. It is evident that this means for the north track a less average tonnage per rail per year; or, in other words, a lower-wheel tonnage. When the load per wheel is less, the resistance and consequently the wear must, necessarily, be less, other things being equal.

"Time of service," also, has an important bearing on the question in hand. It has been only within the last five or six years, as Dr. Dudley has pointed out, that the roadbed of the Pennsylvania Railroad has reached its present admirable condition. Before this time the roadbed was more elastic, more yielding (and probably not uniformly so) than at present. These circumstances might cause a softer rail to be more durable than a harder one, owing to the fact that it would yield more or less to the bending force of the passing load and would thus get a bearing on each cross-tie. The harder rail, on the other hand, being stiffer and more unyielding, would not

have this bearing uniformly, and would thus, to a greater or less degree, be subjected to the same conditions as a beam under shock, vibration, and a rapidly moving load. Under such conditions, I believe, a harder rail would crush, break, and perhaps wear out more easily and quickly than a softer rail. This agrees with Dr. Dudley's statement: "With the improvement in maintenance of way, during the last five or six years, the removal of rails from track from the first two of these causes has quite notably diminished." Under conditions as they now exist on the Pennsylvania Railroad, I believe, the harder rail will give the slower wear. With the ballast comparatively solid and unyielding, as at present, the rail, having a more nearly perfect and uniform bearing, and acting less the part of a beam than that of an anvil must, in my opinion, be a hard one, to withstand the pounding of the locomotive and the abrasion due to combined rolling and sliding friction.

Viewed in this light a hard or soft rail would be respectively preferable as the maintenance of way has become more or less practically perfect. Of the seven rails, in track seven years or less, included in the slower-wearing division, and whose phosphorus units average 40.95, there will be found but one showing, under the same conditions, a slower wear for the softer rail. As these rails were put in track during and after the improvement in maintenance of way, they tend to confirm the proposition that with a well-ballasted track the harder rails give the slower wear. Next under the head of "time of service" comes the consideration of the wheel tonnage (the average tonnage per rail per year in Table I). This, as pointed out by Mr. Ashbel Welch, has been increased over 60 per cent. within the last five or six years. The speed and the weights of locomotives, cars, and trains have also been increased within this period. With the increase of each of these quantities resistance increases. This increased resistance must be overcome by increased friction between drivers and track, which, other conditions being the same, must result in greater or more rapid wear than formerly. Further, with greater speed and weights the defects in the rolling stock—as flat and improperly coned wheels, worn tires, etc.—must cause greater injury to the rail. It must be borne in mind that the average wheel tonnage of the north track is less than that of the south track during the entire period considered.

It will be noticed, in Table I, that the average tonnage per rail (not the average tonnage per rail per year) of the slower-wearing rails is much greater than that of the faster-wearing. I call atten-

tion to this fact because I believe it to be important. We know that the head of a rail when new, is more or less rough, and that this roughness wears off rapidly with the first few million tons of service; consequently, if the loss is determined when the tonnage is low, the loss per million tons will not show the actual wearing rate during its future use. Of course, this influence on the wearing rate becomes less and less, as the tonnage increases. After wearing off this roughness, it may be that the succeeding few million tons cold-roll or hammer the surface, causing it to more successfully resist subsequent wear. From the history of the road it is evident that those rails having a long time of service possess advantages in favor of slow wear. Not only have they a high tonnage, but they also have had a preparation and wear of the surface while the wheel tonnage was light; the later rails have, on the other hand, been subjected from the start to a heavier wheel tonnage with the accompanying conditions unfavorable to slow wear. Upon the relative wear on different grades, and curves, and combinations of the two, it is unnecessary to dwell.

Having tried to make plain the tendency of each of these conditions, I will now tabulate them:

Conditions Favorable to Slower Wear.	Conditions Unfavorable to Slower Wear.
North track. Lighter wheel tonnage. Longer time of service. Greater tonnage. Lighter grade. Lower degree curve.	South track. Heavier wheel tonnage. Shorter time of service. Smaller tonnage. Heavier grade. Higher degree curve.

I do not say these conditions will absolutely determine the relative positions of the 64 rails, because we do not know the ratio of the wear of a given rail under a known set of conditions (favorable, or unfavorable, or both), to the wear of the same rail under another known set of conditions; also because of conditions not given in the data, and because of exceptions named below. But what I have tried to make plain in the preceding remarks is that, in general, the 32 best rails have been in service under conditions, in the main, favorable to slower wear. When this is not the case, the rail, measured by phosphorus units, is hard, and with one exception (rail 915 referred to later), harder than its companion subjected, as far as known, to the same conditions.

To put it more concisely, the slower wear of the 32 best rails is

due to external conditions, which, when summed up, are favorable to slower wear, and not to qualities inherent in the rails themselves; except in a few cases, and these when the rails are hard. If this statement is true, then Dr. Dudley's conclusion, that the slower wear of the 32 best rails is due to their being softer than the 32 faster wearing does not hold.

With the object of learning further what averages of these 64 rails may point out to us, I have arranged them differently, as seen in Table II. We may thus be able to learn whether the indications of the first averages are confirmed or not; or whether the first averages, when studied with the second, may fairly be interpreted to point to, or at least not disprove, conclusions radically differing from those first drawn.

TABLE I.

	North track.	South track.	Total.	Average time of service.	Av. wear per million tons.	Average tonnage per rail.	Average ton per rail, per year.	C.	P.	Si.	Mn.	Phos. units.
All conditions, slower-wearing.	22	10	32	9 y.—5½ in.	.0506	53,737,156	5,686,104	.334	.077	.060	.491	31.3
All conditions, faster-wearing.	10	22	32	6	.1028	40,406,260	6,730,871	.390	.106	.047	.647	38.9

TABLE II.

N. track rails.....	32	0	32	9	5¾	.0638	46,545,146	4,906,225	.329	.071	.064	.508	31.4
S. track rails.....	0	32	32	5	11¾	.0740	47,285,770	7,978,227	.395	.111	.043	.631	38.9

In Table I, which we have just been considering, Dr. Dudley has found that 32 rails of a certain average chemical composition, show a much slower wear than 32 rails of a different average chemical composition. The 32 slower-wearing rails averaging softer than the 32 faster-wearing, the conclusion is drawn that this slower wear is due to this fact. Apparently, this inference is true, but as we have just seen, this slower wear is probably due to other causes. In Table II we have a comparison of rails in the north and south track. This gives us hard and soft rails averaging practically the same chemically as those in Table I, but showing a decided difference in the wear per million tons, comparing the soft and hard of the one with the soft and hard of the other, respectively; also, in the ratio of wear between the soft and hard of each. In Table I this ratio is 1 to 2.03; in Table II, 1 to 1.16. Even this slight difference probably would not have appeared if the north and south rails had

been equal in number in both curves and tangents. By consulting Dr. Dudley's Plates 6 and 7 the distribution will be found to be as follows: In tangents, 19 north track rails and 13 south; in curves, only 13 north track rails and 19 south. As it is, the south track rails with a wheel tonnage (average tonnage per rail per year) $62\frac{1}{2}$ per cent. greater, there is a wear only 16 per cent. greater; and this with nearly equal average rail tonnage in both. With these circumstances in mind we may fairly conclude from Table II that, under the same conditions, the harder rails would give the better wear, which indication Table I does not contradict. This conclusion, like the one before, is in favor of the harder rails.

We have considered the conditions furnished by Dr. Dudley favorable and unfavorable to slower wear. That there are other conditions which materially affect the relative wear of rails will readily occur to all.

Among others, in addition to those given, are the following: Whether the speed is the same over all of the rails; whether the rail is subjected to more than ordinary wear by the stopping and starting of, or by the decreasing or the increasing of the speed of trains, as at or near train and watering stations, switches, crossings, sidings, bridges, tunnels, grades, curves, etc.; whether on tangents both sides of track at the same place are at the same level; whether on curve, the rail is taken near entering tangent or elsewhere; whether the elevation of the outer rail in each case corresponds to the average speed of trains at that curve; whether the character and condition of ballast and roadbed is the same for all rails; and other conditions known to those familiar with maintenance of way.

These conditions, more or less local in their character have, it seems to me, been too little considered in the study of "the wearing capacity of steel rails with relation to their chemical composition and physical properties."

With these circumstances in mind we must agree with Mr. Hunt that "averages are dangerous." In comparing different sets of rails, when, in each set, varying quantities (conditions) too indefinite to be averaged or not averageable are associated with others which are definite and can be averaged, and when the maximum and minimum in one set are greater and less respectively than the maximum and minimum of the other, and where a quantity in one rail differs greatly from the other rails in the same set, making the average to differ widely from any quantity of the same kind in that set, can

averages give conclusions which can reasonably be accepted as true beyond a doubt? I do not think they can. We have seen that widely different conclusions *can* be drawn from indications given by averages made up from different arrangements of the same rails. Indeed, I believe, that conclusions drawn from averages made up from any number of rails under so many and such different conditions would be of value only when confirmed by other data.

To properly study the relative wearing capacity of steel rails with reference to their chemical composition and physical properties, we must compare rails subject to the same conditions, as far as is practical, and which only vary in their chemical composition and physical properties. I have attempted to do this in the following table, made up from Dr. Dudley's level and grade tangents and grade curves. The members of each group have been in the same track the same time, are from the same locality, and have the same grade and curvature (if any); or, in other words, have been subjected to the same conditions, as far as known. It will be noticed that Nos. 893, 920, and 928 of the level and grade tangents have been omitted, and also

No. of group.	No. of rail.	Time of service.	Track.	Grade.	Curve.	Tonnage.	Phos. units.	Loss per yard.	Loss per million tons.	Ratio of Loss. Harder rail = 1.
		Y. M.								
1	881	11 1	North.	92.4		55,546,811	59.2	3.85	.0693	1 : 1.50
	882	11 1	"				28.1	5.78	.1040	
2	883	10 2	"	95.04		52,174,969	35.7	2.54	.0487	1 : .83
	884	10 2	"				32.7	2.13	.0408	
3	885	11	"	89.76		55,197,994	33.9	2.74	.0496	1 : 1.63½
	886	11	"				27.9	4.48	.0811	
4	887	11	"	89.76		55,197,994	24.2	2.13	.0386	1 : 1.61
	888	11	"				21.3	3.44	.0623	
5	889	5 11	South.	21.13		44,620,100	33.1	3.54	.0793	1 : .97
	890	5 11	"				41.7	3.64	.0816	
6	891	7 1	"	40.13		53,687,192	37.7	3.14	.0585	1 : 1.10
	892	7 1	"				33.7	3.46	.0644	
7	894	5 2	"	52.8		38,088,574	45.9	2.93	.0769	1 : 1.03½
	895	5 2	"				45.5	3.03	.0796	1 : 1.04½
	896	5 2	"				40.5	3.04	.0798	
8	913	9 4	North.			45,855,101	53.4	1.01	.0220	1 : 1.31
	914	9 4	"				26.9	1.32	.0288	
9	915	6 1	"			31,514,889	39.2	0.51	.0162	1 : 0.17½
	923	6 1	"				45.5	2.92	.0926	
10	916	6	"			31,127,829	37.5	1.42	.0456	1 : 7.12½
	917	6	"				45.1	0.20	.0064	
11	918	10 6	"			51,720,011	29.9	0.71	.0137	1 : 0.71½
	919	10 6	"				20.3	0.51	.0098	
12	921	5 4	"			27,622,230	37.9	3.05	.1104	1 : 1.50
	922	5 4	"				44.7	2.03	.0735	
13	924	4 1	South.			36,349,989	44.2	1.52	.0418	1 : 1.14
	925	4 1	"				43.8	1.73	.0476	
14	926	9 3	"			76,409,123	30.2	1.53	.0200	1 : 1.13
	927	9 3	"				28.1	1.73	.0226	
15	897	11 2	North (high side).	21.12	5°	52,370,617	33.2	7.31	.1396	1 : .96
	898	11 2	"				28.1	6.99	.1335	
16	899	11 2	North (low side).	21.12	5°	52,370,617	22.3	2.24	.0428	1 : .92
	900	11 2	"				32.2	2.44	.0466	

all of level and grade curves excepting Nos. 897, 898, 899, and 900, because of the impossibility of grouping them in the same manner, no two having the chemical composition and physical properties as the only variables. Phosphorus units have been taken to show the relative hardness.

Of these 16 groups, 10 decidedly indicate slower wear for the harder rail.

Of the remaining 6 groups, three (groups 2, 9, and 15) come well within the limits of error (.25 pound per yard) inherent in the calculation of the data as pointed out by Dr. Dudley; they cannot, therefore, be considered as exceptions. Groups 2 and 15 are but little outside of the limits of error. Rail 923 of group 9 is obviously abnormal. This is probably due to its being overheated, which the chemical analysis shows might easily be the case, and which the physical tests and its low specific gravity tend to confirm. We may say, then, that 13 of these 16 groups fairly indicate, if they do not definitely point out, that, under the same conditions, the harder rail gives the slower wear.

Of course, these comparisons are too few to enable us to arrive at positive conclusions; but indications thus obtained are, I believe, far more valuable and trustworthy than those that averages would give us, made up from any number of rails under many different conditions.

Finally, it seems to me, that the conclusions arrived at earlier in my remarks, together with, and confirmed by the last, show strong evidence that, under the same conditions, the harder rail will give the slower wear.

O. CHANUTE, NEW YORK CITY: We are very much obliged, I am sure, to Mr. Sandberg for his paper upon "Rail Specifications and Rail Inspection in Europe." We have in the United States hitherto been inspecting rails somewhat haphazard, and we are glad to get the results of Mr. Sandberg's long experience.

We recognize that he was among the first, if not the very first, to apply more rational and scientific rules to the designing of iron rails, and, more recently, to adapt these rules to the designing of steel rails, to conform to the capabilities and requirements of this new material. Although we have generally adopted, in this country, sections for steel rails which many of our railway men think even better than those of Mr. Sandberg, he is nevertheless the leader in whose footsteps we have followed ever since it has been established

that the fish-joint was the best method of fastening the rails together at the ends.

We have not, however, yet been able to formulate or to adopt any well-established relation between the height and weight of the rail and the weight and speed of the engines, such as he indicates in his Table No. I. For instance, the New York Central rail is $4\frac{1}{2}$ inches deep, and weighs 65 pounds per yard, while its locomotives are of 37 tons maximum weight. The Erie rail is $4\frac{5}{16}$ inches deep, with 63 pounds weight per yard, and the Pennsylvania rail is $4\frac{1}{2}$ inches deep, with 67 pounds weight per yard, while the maximum weight of locomotives upon both these latter lines is 50 tons. Now which is right? I am inclined to believe that the Pennsylvania Railroad follows the better practice, not because the other rails are too light for the engines, but because inasmuch as steel rails wear out by abrasion, and not by lamination, the Pennsylvania rail promises to be serviceable until about 12 pounds of metal per yard are worn off from the head, while the Erie rail will probably have to be removed from the track when some $8\frac{1}{2}$ pounds are worn off.

Timber is still so cheap with us that we have not hitherto concerned ourselves very greatly about the strength of our rails considered as beams. If after having adopted a rail section, say between 50 and 60 pounds, we have found it a little too limber under increasing weight of locomotives, we have simply put the ties nearer together, and we have thus arrived at the general practice of spacing them about 2 feet between centres, while I notice that Mr. Sandberg's calculations of required stiffness are based upon having the supports 3 feet apart.

We are careful, however, to limit the weight upon our driving-wheels to a maximum of 12,000 pounds (excepting a few experimental locomotives), and when our gradients and trains require more adhesion than can be obtained from the standard "American" engine, we think it better to adopt the "Mogul" type, with 6 drivers, or the "Consolidation," with 8 drivers; the latter having generally an average of but 11,000 pounds per driving-wheel, and being no harder on the rail than other classes of locomotives.

Believing that much of the wear of rails results from undue pressures, I have made some experiments to determine the area of the surfaces in contact between wheels and rails. These were obtained by jacking up a wheel, and introducing between it and the rail a piece of thin tissue-paper, underlaid with a slip of black manifold copying-paper. Upon lowering the wheel, it generally

crushes a hole in the paper, and gives a fair impression of the surfaces in contact. If the wheel and rail were inelastic, this contact would be a mere line, but as they both yield, it becomes a surface which varies with the weight on the wheel and with its condition.

I found that with 11,000 to 12,000 pounds' weight upon a locomotive driving-wheel of about 5 feet diameter the pressures were generally 35,000 to 40,000 pounds to the square inch, although they occasionally ran up much beyond this, but that with 14,000 pounds on a driver the pressures became from 50,000 to 80,000 pounds to the inch, or beyond the elastic limit even of steel.

I also found that under empty freight cars, with say 2400 pounds on a 33-inch wheel, the pressures were generally 20,000 to 30,000 pounds per square inch; that with the car loaded with 11 tons, increasing the weight to say 5150 pounds per wheel, these pressures became about 35,000 pounds to the inch, while if the car was loaded with 20 tons, thus giving 7400 pounds per wheel, the pressures increased to 50,000 or 60,000 pounds to the square inch.

As we increase the weight upon our cars, therefore,—and I believe this to be the correct and inevitable practice,—we must be prepared to find our steel rails wear out faster than they hitherto have done. We may, perhaps, reduce the pressure by increasing the diameter of car-wheels, but my own judgment is that we should endeavor to limit the weight on locomotive-drivers of 5 feet diameter to 12,000 pounds, and on 33-inch car-wheels to about 7000 pounds, so as not to bring crushing strains upon our rails and wheels.

I notice that Mr. Sandberg is disposed to think that our adoption of 30 feet as a normal rail length is an extreme limit. I believe, however, that our mills have found no difficulty in working up to this, and that we get only about 3 per cent. of shorter rails, under the provision that not more than 10 per cent. may be delivered under 30 feet, down to 25 feet. The difficulties which he mentions, as connected with ocean transportation of 30-foot rails, need not concern us much at the present time. I believe that the iron rail mills of this country have the capacity for turning out about 1,000,000 of tons a year, if so many tons of iron rails were called for, and that the steel mills have a present capacity of about 1,500,000 tons, and a prospective capacity, by the end of this year, of some 1,750,000 tons of steel rails per annum. Now this would furnish us enough rails, if fully employed, to lay or to relay 25,000 to 27,000 miles of track a year. We now have 93,000 miles of railway, of which about 60,000 miles are ten years old

and over. Allowing for the postponement of renewals in past years, double tracks, etc., I estimate that these railways will require some 800,000 to 900,000 tons of rails per annum for the next four or five years, to relay their tracks. We are also building some 7000 miles of new railway a year, a rate of progress, however, which I believe we cannot maintain without great risk of running into unprofitable investments, and bringing about a fresh collapse; but even if we do build 7000 miles a year, the aggregate demands for rails in the United States, would not exceed 1,500,000 or 1,600,000 tons a year, or a little less than the estimated capacity of the steel works alone for 1882.

We are not likely, therefore, to import many rails from Europe, except occasionally on an emergency, and as a reminder to our manufacturers that there are other railmakers in the world; but if we do, let us not ask the foreign mills to grind off the ends of the rails, to make them exactly of even length, a foreign practice which Mr. Sandberg so justly warns us against. Neither shall we ask them to notch steel rails, except in rare instances, as most of our roads have now adopted, or are adopting, angle fish-plates, to which the notching is transferred, thus preventing creeping through the shearing resistance of the bolts; but we shall undoubtedly require them to drill all holes for the latter, and we find that a round hole, one inch in diameter, with a $\frac{3}{4}$ inch bolt, allows sufficient play to provide for contraction and expansion.

I made some experiments upon rail joints some years ago, which indicated rather better results than those given in Mr. Sandberg's Table No. I, Appendix, II. I found that the Erie standard steel rail, of 63 pounds weight per yard, upon solid bearings two feet apart in the clear, required the application of a weight of 60 tons on the head in the centre between the bearings to break it; that the old joint, composed of two flat plates, broke with 20 tons similarly applied; that the composite joint, consisting of one flat plate and one angle plate, broke with a weight of 25 tons, while the Erie standard joint, of two angle-plates 24 inches long, required 34 tons to break it. The flat-plate fishing was, therefore, 33 per cent., the composite joint 41 per cent., and the standard angle-joint 57 per cent., as strong as the solid rail, and the angle-plate fish showed such marked superiority, that our adoption of it was fully confirmed.

But we are especially obliged to Mr. Sandberg, for the details and blank forms which he gives us of his method of inspection. I wish particularly to call your attention to the blank for the inspection

book, Appendix IV, and to the form for reports, Appendix V. I think it would be well for us to adopt them for the use of our own inspectors in this country.

I do not, however, quite understand that clause in his specification for *steel* rails (page 30), which, under the head of "Tests," provides that: "2d, The rails must carry, in the same position, a load of — tons without breaking; after this the flange of the rail will be cut, and the rail broken. The fracture must show perfect welding, especially in the head." I thought it was a peculiarity of steel rails, that they were made from a single piece or ingot, and I am puzzled to imagine how the foreign makers contrive to get welds into them.

You will notice that nearly all my remarks refer to steel rails. I ought to have said so before, but the fact is, that when we now talk or think of rails, it is almost always of steel rails, for the days of iron rails are numbered. Already we see, when we examine one of these diagrams of prices of iron and steel rails, which look so much like the profile of a railway preliminary survey through a mountainous country, that the iron rails average only \$5 or at most \$10 a ton cheaper than steel rails; and the time cannot be far distant when steel rails will be produced as cheaply as iron. Indeed, I do not believe that any of our roads are now so poor as to be able to afford to buy iron rails, except, as I said before, upon an emergency.

The thanks and support of all railway men are therefore due to Dr. Dudley for his resolute attempt to ascertain the best composition and characteristics for steel rails. He may not as yet have gathered all the necessary data; his present conclusions may have to be corrected with reference to further facts; but I know that he is rendering valuable service, and I believe that he is on the right track.

I quite agree with the remark made by Mr. William Sellers, that the consumer should not undertake to prescribe to the manufacturer how he is to make his rails, nor what materials he is to employ, but should leave him free to select the surest and cheapest way of making a good article. The consumer is interested in the results only; but as the desired result in this case is that the rail shall wear as long as possible, and as steel rails wear out so slowly that we cannot know for many years which make of them is going to give the very best satisfaction; and while the economical results are so important, I believe that it is our duty to endeavor to ascertain the characteristics of the best rails, to assist the manufacturer to repeat his successes,

and to avoid his failures, by giving him whatever data we can gather as to the rails in our tracks.

This, as I understand, is what Dr. Dudley has undertaken to do, by ascertaining the chemical composition and physical characteristics of the rails which have best or worst worn on the Pennsylvania Railroad. While I will presently mention some considerations why his experiments may need to be revised, I yet recognize that he has done and is doing a great public service.

I must say, however, that the chemical composition which he recommends, does not strike me as a particularly soft steel. From the criticism which he has received here, I doubt whether Dr. Dudley himself now thinks that he has as soft a thing as he at first imagined. He advises that the phosphorus should not exceed 0.10 of one per cent., the silicon not above 0.04, and that the carbon should aim at 0.30, and the manganese at 0.35 of 1 per cent.

Now the Erie specification of 1876, adopted after consultation with Mr. Holley, reads: "Not less than $\frac{3.0}{100}$ ths nor more than $\frac{4.5}{100}$ ths of 1 per cent. of carbon; not more than $\frac{1.0}{100}$ ths of 1 per cent. of phosphorus, and not more than $\frac{1.6}{100}$ ths of 1 per cent. of phosphorus and silicon taken together. It may contain manganese, but shall be substantially free from other impurities."

In view of the fact that this was the state of the art about the time Dr. Dudley began his labors, and that really *soft* steel, that which we use for our boiler plates, only contains 0.08 to 0.15 of 1 per cent. of carbon, the term of "soft steel" which is much dwelt upon by the author, is rather a misnomer. What he is engaged upon is the ascertaining what are the exact chemical compositions which gives absolutely the best rails, and how these shall be distinguished by physical tests. To accomplish this satisfactorily, he will have to gather a good many more data.

I think exception may be taken to the method by which Dr. Dudley has undertaken to ascertain the loss of weight sustained by each rail. Having taken up the whole rail, presumably about 30 feet long, he has cut out a slice from it, somewhere, $\frac{1}{2}$ an inch thick, and from this slice he has, by weighing and measuring, ascertained the loss of weight. Any one who will caliper a worn rail throughout its whole length, and thus ascertain how much greater is the wear in some spots than in others (differing $\frac{1}{16}$ of an inch in sections 2 inches apart in many cases), will have serious doubts whether Dr. Dudley has in each case hit upon the particular half inch which is a fair representation of that wear.

It seems also difficult to accept the inference to which this method of procedure leads him, when he says that "rails rolled at the same mill, at the same time, and with the same thickness of web, and same shape of foot, differed from each other in the original weight (*as computed*), from $1\frac{1}{2}$ to 3 pounds per yard." Such is not our experience with Erie rails. We find that when the rolls are freshly turned up, the rails run about $62\frac{3}{4}$ pounds per yard, and that this weight is gradually increased as the rolls wear, to about $63\frac{1}{4}$ pounds per yard; each invoice of say 1000 tons (the shipments are generally of about this amount), averaging as near as may be the 63 pounds per yard represented by the standard template. Here, therefore, we have a variation of only $\frac{1}{2}$ pound per yard, which is the limit assigned by our specification, and as I said before, it seems hard to believe that on the Pennsylvania Railroad, it could have been so much as $1\frac{1}{2}$ to 3 pounds per yard.

I scarcely need to point out that if errors have thus crept into Dr. Dudley's estimates of the loss of weight sustained by each rail, his reasoning and conclusions will be affected throughout. I recognize the difficulty of getting at the wear of a rail the exact original weight of which is not known, but I believe Dr. Dudley will yet find better methods than that of computing it from a half-inch slice. Perhaps more satisfactory results may be reached by a careful caliper of the stem, head, and foot of the rail, and ascertaining its density, from which to deduce its original weight, deducting therefrom, to ascertain the wear, the actual weight of the worn rail. In fact, as he finally resorted to the method of averages, Dr. Dudley would have reached nearly the same result by assuming that the rails originally averaged of standard weight, and weighing together each group of eight rails, upon which he bases his deductions of wear.

I may also say a word as to the comparisons of wear upon the upper and lower sides of curves. These would have been more satisfactory if we had been told the differences, if any, which exist between the elevation of the outer rail on these various curves; also, the speeds at which trains generally run over them. The elevation of the outer rail being intended to overcome the centrifugal force, and this, of course, varying with the speed of the train, it is quite practicable for the track foreman to throw the wear upon the inner or the outer rail, by changing the elevation, or, in a less degree, for the locomotive engineer to do the same thing, by running faster or slower than the speed for which the curve is elevated. I hope, how-

ever, that Dr. Dudley will check over and continue his investigation. It is not improbable that the result will be still further to confirm his theory.

The railroad men of the whole country, who are specially interested in reaching sound conclusions on this subject, can materially assist in gathering additional data, by taking care to preserve rails which have worn exceptionally well or ill in their tracks, and sending them, with a statement of the particulars of each case, either to Dr. Dudley, if he will consent to test them, or to some of the Bessemer works from which they obtain their steel. All of these have competent chemists; they are vitally interested in maintaining a reputation for making good steel rails, and they would doubtless be glad to make arrangements to analyze and test any specimen rail which might be sent to them, in order to ascertain the causes of its excellence or deficiencies.

In listening to Dr. Holley's paper upon Rail Sections, I was reminded of De Quincey's ideal murderer, who, beginning with a murder which he thought little of at the time, had gradually fallen to robbing, drinking, and Sabbath breaking, and so on, down to incivility and procrastination. For having adopted some years ago a rail pattern which I have never recommended to other roads, nor claimed credit for, I now unexpectedly find from Dr. Holley's paper that 62 per cent. of modern rail sections are fashioned after that pattern, that the considerations which guided me are thought worth enumerating, and that it furnishes a good text from which to preach a sermon to railroad men. I hope, however, to satisfy you that I am not entitled to as much notice as Dr. Holley has been pleased to give me.

As Mr. Welch has told you, we were both in 1874 members of a committee of the American Society of Civil Engineers to investigate the best form of rail sections. He then called my attention to the success of the thin flange and stem of his pattern of 1866, and I adopted them for the Erie Railway, which was then much in need of a standard steel rail section. The bevelled head was furnished, ready made, by the sections of old rails which I examined, and was confirmed by the templates of worn wheels, twenty or thirty in number, which I obtained from locomotives and cars. I simply gathered the data, and was guided by them, as any one would have been in my place, and as in fact others had been, for I am informed that Mr. Sayre, of the Lehigh Valley Railroad, and Mr. Fritz, of the

Bethlehem Steel Works, had designed and rolled a similar rail, as early as 1870.

The Erie rail was originally designed to weigh sixty pounds per yard, this being the limit at that time imposed by the managers of the road. It took some months to get the pattern accepted, some of the rolling-mill managers declaring that it could not be rolled without producing a large percentage of imperfect rails. Mr. L. S. Bent, however, the superintendent of the Pennsylvania Steel Company's Works, thought differently and determined to try it. He found that the percentage of imperfect rails was actually less than with other patterns then in use, and he designed and introduced a number of steel rail sections on the same principle, which have become standards.

Some three or four thousand tons were rolled and laid of the original Erie sixty-pound pattern, and they have stood very well, but Dr. Holley having suggested that the thinness of the foot, in proportion to the head, might cause dangerous internal strains in cooling, and thus make the rail brittle and dangerous, a thickness of one-sixteenth of an inch was added to the foot, increasing the weight to sixty-three pounds per yard, and this has been the Erie standard section ever since. As I said before, in my opinion the Pennsylvania Railroad section of sixty-seven pounds per yard is better, as likely to wear about 50 per cent. longer.

Up to a certain point, there is an advantage in diversity of railroad practice. So long as the best device for a particular purpose is not ascertained, there is a necessity for experimenting, and the resulting variety of design. When, however, the best pattern is approximately agreed upon, the effort should be towards uniformity. This point seems now to have been reached about steel rail sections, although I had no idea this was the fact, and I hope the railroads will take advantage of the economy which Dr. Holley has shown us to result from the adoption of uniform standards.

He has called our attention to the importance of uniformity in fishing, and especially in spacing the bolt-holes, but he has not told us which he considers the best practice. I venture to present a drawing of the Erie standard joint. (See accompanying plate.) There is nothing novel about it, but the points which we think meritorious, are the following :

1st. The holes in the rails are placed as far from the end as we deemed practicable. The centre of the first hole is pitched an even 4 inches from the end of the rail, and the second hole is 6 inches beyond this, or 10 inches from the end.

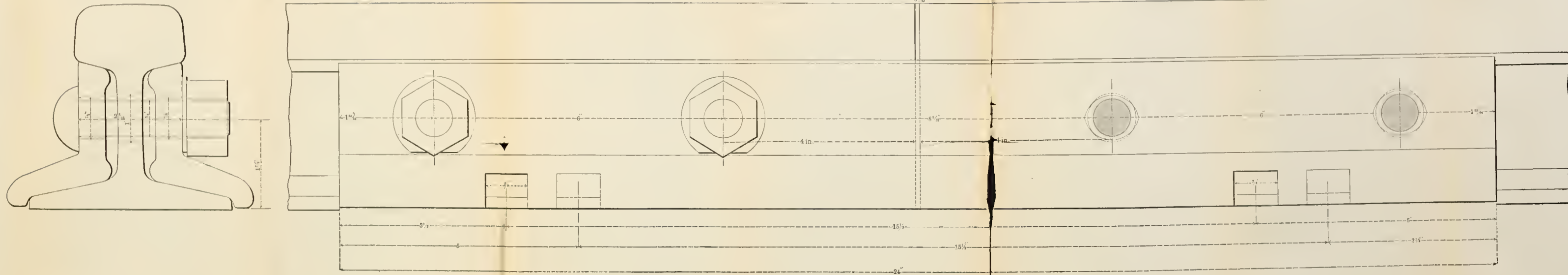
Quinto

6"

$7\frac{1}{3}$ "



STANDARD RAIL JOINT, - N.Y.L.E. & W.R.R. FULL SIZE



2d. These holes are drilled in all cases, are 1 inch in diameter, and as near the neutral axis of the rail as we could get.

3d. The fishing is done with angle plates, which we find about 70 per cent. stronger than flat plates. The notching which is in the fish-plate and not in the rail, is spaced $3\frac{1}{2}$ inches from one end, and 5 inches from the other, so that when the plate, which is reversible, is applied to both sides of the rail, the notches are staggered sufficiently to avoid splitting the tie with the spikes.

4th. The allowance for expansion is made in the fish-plate, the two centre holes being spaced $8\frac{9}{32}$ inches apart. As the next holes are, of course, 6 inches beyond the centre holes, and the plate is designed to be 24 inches long, it will be noticed that if the man at the shears cuts it off of the right length, and the man at the punch centres it exactly, the distance from the centre of the end holes to the end of the plate will be precisely $1\frac{55}{64}$ inches. I hope that Dr. Holley, who says he was appalled at the thought that the mind of man can hit perfection in spacing fish-plate holes within the 64th of an inch, will see from this brief exposition of the process, that it is more easy to accomplish than he supposed.

5th. The holes in the fish-plate are made oval to allow for expansion and contraction. The bolt, which is $\frac{3}{4}$ of an inch in diameter, is upset under the head to fill this oval hole, and thus prevent turning. It is provided with a hexagonal nut, under which we generally place a thin washer of wrought iron. We have very little trouble from nuts getting loose, so little indeed, that while we have experimented with a number of lock-nuts, we have not deemed it necessary to adopt any of them.

But I fear I am becoming wearisome by my discussion of these details, which would be more appropriate before a special committee on this subject, such as that appointed in 1874, by the Society of Civil Engineers. The main point before you, is that so well made by Dr. Holley, of the importance and economy of uniformity in rail sections and fastenings. Of that we have had some experience. We had on the Erie railway, when the new steel section was adopted, 12 patterns of steel rails, 29 patterns of iron rails, and 96 different styles of fastenings. These caused no end of annoyance, delays, and expense, in matching or mismatching them, taking up and changing about long strings of rails, and in the large stocks which it was necessary to keep for repairs. This has all been done away with, by the adoption of a single pattern of rail and of fastening, and the resulting economy fully confirms all that Dr. Holley has said. He

has shown us that the railroads of this country can save several millions a year by adopting uniform rail sections, and as unfortunately for him, he cannot patent his idea, it only remains for the railroads to adopt it, and to thank him for his paper.

DR. C. B. DUDLEY, ALTOONA, PA.: In rising to close this interesting discussion, I want, in the first place, to thank every one who has contributed to it for his full and open criticism. The work which has been done on steel rails, and which has been discussed here during these two days, was not done to establish any pet theories, nor to make out that any person was great or any person small, but with a sincere desire to get at what is the truth in regard to the wearing capacity of steel rails. There are enormous commercial considerations involved in this question, and, as I look at the matter, the more honest criticism and fair discussion there is, the more likely it is that the truth will appear.

And first I would like to say that it seems to me very little has been said here upon the main conclusion of the paper, namely, that the softer rails give the better wear. All sorts of side issues have been discussed; but this point, which is really the principal one at issue, has been largely ignored, and I cannot but feel that it still remains unshaken.

With regard to chemists and chemical work, there has been considerable said tending to throw discredit on chemists and their work; and while I believe that there have been in the past, are now, and may be in the future, a good many poor chemical analyses made, I also believe that chemists are, as a rule, as honest and competent as gentlemen who belong to other professions. There are chemists who are chemists, and chemists who are not chemists.

The determination of manganese has been called in question. Now I think the chemist at almost every steel works in the country will tell you that, in his experience, the manganese differs in different parts of the same ingot. Mr. T. T. Morrell, chemist of the Cambria Iron Company, whom I believe to be a thoroughly competent and honest chemist, tells me that he has often found different amounts of manganese in different parts of the same ingot. Come with me to Altoona, and I will take you into the machine shop where steel is being cut and shaped, and I will show you that it is often necessary to stop the lathe or planer and take a cold chisel to cut out a hard spot, or else run the risk of breaking the tool. This hard spot is simply a part of the spiegel which is not thoroughly mixed with the mass

when the steel is made. In the rapid methods by which steel is at present manufactured, time enough is not allowed for the spiegel to become uniformly mixed. What wonder, then, that chemists find different amounts of manganese in what is supposed to be, but is not, the same steel. Indeed I believe it is possible for the borings from one bore hole in the same ingot to be given to two chemists and to have them find different amounts of manganese, and yet both analyses be correct. And so, I say to the steel makers, "Make uniform steel, and we, as chemists, will tell you what there is in it."

With regard to the determinations of manganese in the series of rails we are discussing, I would say, I wish Mr. Wells was here, that you might see him for yourselves. When I began this work, I wrote to my old instructor in chemistry, Professor O. D. Allen, of the Sheffield Scientific School, to recommend me some one to help me. He replied that if Mr. Wells would come he could heartily recommend him. He had had two years' experience since his graduation, and, said Professor Allen, "I regard him as the best analytical chemist that has graduated under me." And I may add that both Professor Drown and myself graduated under Professor Allen.

Still further, it is simply impossible that any errors, either in the chemical analyses or the physical tests, should have had any influence in establishing the point that the softer steel gives the better wear. This follows from the way in which the work was done. First the physical tests were made, then the analyses, then the tonnage was computed, and finally the loss of metal was determined. So that we knew nothing about a rail until all the chemical analyses and physical tests were made. Furthermore, some of the rails that were selected as faster-wearing rails, when we came to get the rate of wear, were found to be slower-wearing rails. So that no previous bias of mind, or, as it seems to me, no possible errors in the work could influence the result.

Again with regard to sulphur and copper, it is said that these are of vital importance, and should have been determined. In answer

this I would say: Where is the man that can affirm, and back his statement by any analysis, that sulphur and copper have any influence on the wearing capacity of steel? I do not say that these elements do not have an influence on wear, but when this investigation was started the best information that I could get was that sulphur and copper were of vastly more importance to the steel manufacturers than they were to the consumer. And so I say that I believe the sulphur and copper are of importance to the makers of

steel, but of not so much importance to one studying its wearing capacity. If you want to know the sulphur and copper in these rails you may determine them.

Probably no one has thought over the question why some of the rails in this series seem to be exceptions to the general law more than I have. This suggestion in regard to sulphur and copper, and other undetermined substances, and especially, in my judgment, oxide of iron, furnishes a possible solution of the problem. If we knew every foreign substance which these rails contain, I doubt not but that some of the anomalies would be explained. And I would here like to ask chemists who have time to devote to such studies, to give us a method for determining oxide of iron in steel.

Another point made was the influence of heavier locomotives and cars on the wear of rails. If I understand this criticism it is this: Your slower-wearing rails had lighter-wheel tonnage for at least a portion of their life—the earlier portion—while your faster-wearing rails have had almost altogether heavier-wheel tonnage. In reply, I say the slower-wearing rails had during the latter part of their life the same heavier-wheel tonnage that the faster-wearing had. All the rails were taken out of the track at the same time, and, consequently, so far as I can see, the comparison of the wearing capacity of steel with its quality is strictly a fair one.

Again, in the course of this discussion—not a few times—the exceptional cases, the cases where individual rails did not conform to the general law, have been taken out and held up prominently before us, as though these individual and exceptional cases were the only thing we should consider. Now, I submit to you that this is simply trying to overthrow a law by the exceptions to it, or, in other words, to nullify the teachings of a large number of samples by the teachings of a few exceptional cases, and I submit still further that this method of proceeding is neither good logic, nor fair, sound deduction.

I must not omit to comment on the remarks of those speakers who have refuted conclusions which I did not advance, and have then considered my position, namely, that the softer rails give the better wear, as completely demolished. A notable case of this kind is Mr. Kent, who because he does not find that there is a direct relation between the loss of metal and the carbon, phosphorus, silicon, or manganese, or phosphorus units in this series of rails, affirms that I have not solved the whole problem of wear, and, *ergo*, the softer rails do not give the better wear. I beg to remind him that I have never said that I had solved the problem of wear. I ex-

I say I have not solved it, but I do not see how that affects the main question; nor do I see, because there is no direct relation between carbon and loss of metal, that it is impossible for me to take a series of rails which have been in service and find by a study of them what chemical composition and what physical properties are, in general, characteristic of those rails which have given the best service. This I claim to have done, and the conclusion seems to me so plain that he who runs may read, namely, that the softer rails give the better wear.

With regard to Mr. Metcalf and his attributing all the troubles of steel to nitrogen, I think it may fairly be said, first, that Mr. Metcalf brings no proof to show that nitrogen is the bane of steel; and second, if it is, the natural conclusion would be that no steel could be made except by the crucible process, which would undoubtedly be a satisfactory conclusion for crucible steelmakers, like Mr. Metcalf, but would hardly satisfy the stockholders of the Bessemer works, or stop their making steel with nitrogen in it in the future. With regard to another criticism of Mr. Metcalf's, that the question of flow had not been considered, I would say that I think there is very little evidence of flow in this series of rails anyway. And so I asked Mr. Metcalf how the flow influenced the loss of metal by wear. He replied that flow squeezes the metal toward the flange and then the flange rubs it off. To this I made reply, that the flow, whatever there is of it, must be away from the forces which produce it. Now, both the pressure of the flange against the rail and the coning of the wheels would cause the metal to flow away from the flanges instead of toward them, and consequently I do not see how you are going to get metal there for the flange to rub off. The flow must be in the other direction, or away from the flanges. Although a few of the rails in this series give evidence of having a little metal pushed off out of place by reason of flow, yet the metal is there. It is not worn off, and the question we are studying is loss of metal by wear.

One or two points further, and I am done. It has been said, "You have not exhausted the question yet. More study must be put upon it." No one is more conscious of the truth of these statements than I. I do not pretend to have exhausted the question. I wish there were fifty workers in this field. But I believe that the results that we are discussing are the best information that we now have upon the question as to the relation between the wearing capacity of steel and its chemistry and physics. I would not at all affirm

that this will be the best information on the subject five years from now. But I think that man does his life-work best who lives in all the light that he has in his own time. And so I ask you to utilize this work, to act upon it, and guide your practice by it until something better is obtained.

Finally, I have been accused of trying to teach the steelmakers how to make steel, and it is to be supposed that they know already much more about that point than I do. Now, if any one thinks that such has been my aim, or has ever been in my thoughts, he has certainly misunderstood me. What I am striving for is to tell the steelmakers what we want, not how to supply this want. This whole question of the fitness of material for the purposes for which it is intended is in its infancy. We are doing something toward studying it at Altoona. The principle which governs us there is that the kind of service that is to be required of the metal must determine what kind of metal shall be used. Because softer steel gives better rails, we do not think softer steel will give better crank-pins. In crank-pins we require stiffness, which comes with harder steel. But in rails, in tires, in bridge rods, and in boiler plate we are, so far as our knowledge now goes, inclined toward soft steel. And all the information which we have thus far been able to accumulate in regard to these kinds of service confirms our position and justifies our conclusion.

And now, how can the best results be obtained in trying to decide upon the quality of material best fitted for any kind of service? I do not see that the steelmakers can study this question alone, for after the steel leaves their hands they know very little of its behavior. It does not come under their personal observation and study. It seems to me, therefore, that the question can only be studied by both the consumer and the producer working together. I cannot but regard that the interests of the consumer and the producer in this matter are one, that neither can solve the question alone, and so I ask you to work with, rather than oppose me, to utilize the information that is gained, so far as it is gained, and to constantly hold in mind the necessary dependence of both producer and consumer upon each other.